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ENTRANCE TO THE ROYAL SOCIETY ROOMS, BURLINGTON HOUSE, LONDON

A HISTORY OF SCIENCE

BY
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IN FIVE VOLUMES

VOLUME V.

ASPECTS OF RECENT SCIENCE

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A HISTORY OF SCIENCE

BOOK V

ASPECTS OF RECENT SCIENCE

STUDENTS of the classics will recall that the old Roman historians were accustomed to detail the events of the remote past in what they were pleased to call annals, and to elaborate contemporary events into so-called histories. Actuated perhaps by the same motives, though with no conscious thought of imitation, I have been led to conclude this history of the development of natural science with a few chapters somewhat different in scope and in manner from the ones that have gone before.

These chapters have to do largely with recent conditions. Now and again, to be sure, they hark back into the past, as when they tell of the origin of such institutions as the British Museum, the Royal Society, and the Royal Institution; or when the visitor in modern Jena imagines himself transplanted into the Jena of the sixteenth century. But these reminiscent

moods are exceptional. Our chief concern is with strictly contemporary events—with the deeds and personalities of scientific investigators who are still in the full exercise of their varied powers. I had thought that such outlines of the methods of contemporary workers, such glimpses of the personalities of living celebrities, might form a fitting conclusion to this record of progress. There is a stimulus in contact with great men at first hand that is scarcely to be gained in like degree in any other way. So I have thought that those who have not been privileged to visit the great teachers in person might like to meet some of them at second hand. I can only hope that something of the enthusiasm which I have gained from contact with these men may make itself felt in the succeeding pages.

It will be observed that these studies of contemporary workers are supplemented with a chapter in which a hurried review is taken of the field of cosmical, of physical, and of biological science, with reference to a few of the problems that are still unsolved. As we have noted the clearing up of mystery after mystery in the past, it may be worth our while in conclusion thus to consider the hordes of mysteries which the investigators of our own age are passing on to their successors. For the unsolved problems of to-day beckon to the alluring fields of to-morrow.

I

THE BRITISH MUSEUM

IN the year 1753 a remarkable lottery drawing took place in London. It was authorized, through Parliament, by "his gracious Majesty" King George the Second. Such notables as the archbishop of Canterbury and the lord chancellor of the realm took official interest in its success. It was advertised far and wide—as advertising went in those days—in the *Gazette*, and it found a host of subscribers. Of the fifty thousand tickets—each costing three pounds—more than four thousand were to be of the class which the act of Parliament naïvely describes as "fortunate tickets." The prizes aggregated a hundred thousand pounds.

To be sure, state lotteries were no unique feature in the England of that day. They formed as common a method of raising revenue in the island realm of King George II. as they still do in the alleged continental portion of his realm, France, and in the land of his nativity, Germany. Indeed, the particular lottery in question was to be officered by the standing committee on lotteries, whose official business was to "secure two and a half million pounds for his Majesty" by this means. But the great lottery of 1754 had interest far beyond the common run, for it aimed to meet a national need of an anomalous kind—a purely intellectual

need. The money which it was expected to bring was to be used to purchase some collections of curiosities and of books that had been offered the government, and to provide for their future care and disposal as a public trust for the benefit and use of the people. The lottery brought the desired money as a matter of course, for the "fool's tax" is the one form of revenue that is paid without stint and without grumbling. Almost fifty thousand pounds remained in the hands of the archbishop of Canterbury and his fellow-trustees after the prizes were paid. And with this sum the institution was founded which has been increasingly famous ever since as the British Museum.

The idea which had this splendid result had originated with Sir Hans Sloane, baronet, a highly respected practising physician of Chelsea, who had accumulated a great store of curios, and who desired to see the collection kept intact and made useful to the public after his death. Dying in 1753, this gentleman had directed in his will that the collection should be offered to the government for the sum of twenty thousand pounds; it had cost him fifty thousand pounds. The government promptly accepted the offer—as why should it not, since it had at hand so easy a means of raising the necessary money? It was determined to supplement the collection with a library of rare books, for which ten thousand pounds was to be paid to the "Right Honorable Henrietta Cavendish Holles, Countess of Oxford and Countess Mortimer, Relict of Edward, Earl of Oxford and Earl Mortimer, and the Most Noble Margaret Cavendish, Duchess of Portland, their only daughter."

The purchases were made and joined with the Cottonian library, which was already in hand. A home was found for the joint collection, along with some minor ones, in Montague Mansion, on Great Russell Street, and the British Museum came into being. Viewed retrospectively, it seems a small affair; but it was a noble collection for its day; indeed, the Sloane collection of birds and mammals had been the finest private natural history collection in existence. But, oddly enough, the weak feature of the museum at first was exactly that feature which has been its strongest element in more recent years—namely, the department of antiquities. This department was augmented from time to time, notably by the acquisition of the treasures of Sir William Hamilton in 1773; but it was not till the beginning of the nineteenth century that the windfall came which laid the foundation for the future incomparable greatness of the museum as a repository of archæological treasures.

In that memorable year the British defeated the French at Alexandria, and received as a part of the conqueror's spoils a collection of Egyptian antiquities which the savants of Napoleon's expedition had gathered and carefully packed, and even shipped preparatory to sending them to the Louvre. The feelings of these savants may readily be imagined when, through this sad prank of war, their invaluable treasures were envoyed, not to their beloved France, but to the land of their dearest enemies, there to be turned over to the trustees of the British Museum.

The museum authorities were not slow to appreciate the value of the treasures that had thus fallen into their

hands, yet for the moment it proved to them something of a white elephant. Montague Mansion was already crowded; moreover, its floors had never been intended to hold such heavy objects, so it became imperatively necessary to provide new quarters for the collection. This was done in 1807 by the erection of a new building on the old site. But the trustees of that day failed to gauge properly the new impulse to growth that had come to the museum with the Egyptian antiquities, for the new building was neither in itself sufficient for the needs of the immediate future nor yet so planned as to be susceptible of enlargement with reasonable architectural effect. The mistakes were soon apparent, but, despite various tentatives and "meditatings," fourteen years elapsed before the present magnificent building was planned. The construction, wing by wing, began in 1823, but it was not until 1846 that the last vestige of the old museum buildings had vanished, and in their place, spreading clear across the spacious site, stood a structure really worthy of the splendid collection for which it was designed.

But no one who sees this building to-day would suspect its relative youth. Half a century of London air can rival a cycle of Greece or Italy in weathering effect, and the fine building of the British Museum frowns out at the beholder to-day as grimy and ancient-seeming as if its massive columns dated in fact from the old Grecian days which they recall. Regardless of age, however, it is one of the finest and most massive specimens of Ionic architecture in existence. Forty-four massive columns, in double tiers, form its frontal colonnade, jutting forward in a wing at either end. The

ENTRANCE TO THE BRITISH MUSEUM, LONDON

flight of steps leading to the central entrance is in itself one hundred and twenty-five feet in extent; the front as a whole covers three hundred and seventy feet. Capping the portico is a sculptured tympanum by Sir Richard Westmacott, representing the "Progress of Civilization" not unworthily. As a whole, the building is one of the few in London that are worth visiting for an inspection of their exterior alone. It seems admirably designed to be, as it is, the repository of one of the finest collections of Oriental and classical antiquities in the world.

There is an air of repose about the *ensemble* that is in itself suggestive of the Orient; and the illusion is helped out by the pigeons that flock everywhere undisturbed about the approaches to the building, fluttering to be fed from the hand of some recognized friend, and scarcely evading the feet of the casual wayfarer. With this scene before him, if one will close his ears to the hum of the great city at his back he can readily imagine himself on classical soil, and, dreaming of Greece and Italy, he will enter the door quite prepared to find himself in the midst of antique marbles and the atmosphere of by-gone ages.

I have already pointed out that the turning-point in the history of the British Museum came just at the beginning of the century, with the acquisition of the Egyptian antiquities. With this the institution threw off its swaddling-clothes. Hitherto it had been largely a museum of natural history; in future, without neglecting this department, it was to become equally important as a museum of archæology. The Elgin marbles, including the wonderful Parthenon frieze, con-

firmed this character, and it was given the final touch by the reception, about the middle of the century, of the magnificent Assyrian collection just exhumed at the seat of old Nineveh by Mr. (afterwards Sir Henry) Layard. Since then these collections, with additions of similar character, have formed by far the most important feature of the British Museum. But in the mean time archæology has become a science.

THE MUSEUM OF NATURAL HISTORY

Within recent years the natural history collection has been removed *in toto* from the old building to a new site far out in South Kensington, and the casual visitor is likely to think of it as a separate institution. The building which it occupies is very modern in appearance as in fact. It is a large and unquestionably striking structure, and one that gives opportunity for very radical difference of opinion as to its architectural beauty. By some it is much admired; by others it is almost equally scoffed at. Certain it is that it will hardly bear comparison with the parent building in Great Russell Street.

Interiorly, the building of the natural history museum is admirably adapted for its purpose. Its galleries are for the most part well lighted, and the main central hall is particularly well adapted for an exhibition of specimens, to which I shall refer more at length in a moment. For the rest there is no striking departure from the conventional. Perhaps it is not desired that there should be, since long experience seems to have settled fairly well the problem of greatest economy of space, combined with best lighting fa-

cilities, which always confronts the architect in founding a natural history museum.

There is, however, one striking novel feature in connection with the structure of the natural history museum at Kensington which must not be overlooked. This is the quite unprecedented use of terra-cotta ornamentation. Without there is a striking display of half-decorative and half-realistic forms; while within the walls and pillars everywhere are covered with terra-cotta bas-reliefs representing the various forms of life appropriate to the particular department of the museum which they ornament. This very excellent feature might well be copied elsewhere, and doubtless will be from time to time.

As to the exhibits proper within the museum, it may be stated in a word that they cover the entire range of the faunas and floras of the globe in a variety and abundance of specimens that are hardly excelled anywhere, and only duplicated by one or two other collections in Europe and two or three in America.

It would be but a reiteration of what the catalogues of all large collections exhibit were one to enumerate the various forms here shown, but there are two or three exhibits in this museum which are more novel and which deserve special mention. One of these is to be found in a set of cases in the main central hall. Here are exhibited, in a delightfully popular form, some of the lessons that the evolutionist has taught us during the last half-century. Appropriately enough, a fine marble statue of Darwin, whose work is the fountain-head of all these lessons, is placed on the stairway

just beyond, as if to view with approval this beautiful exemplification of his work.

One of these cases illustrates the variations of animals under domestication, the particular specimens selected being chiefly the familiar pigeon, in its various forms, and the jungle-fowl with its multiform domesticated descendants.

Another case illustrates very strikingly the subject of protective coloration of animals. Two companion cases are shown, each occupied by specimens of the same species of birds and animals—in one case in their summer plumage and pelage and in the other clad in the garb of winter. The surroundings in the case have, of course, been carefully prepared to represent the true environments of the creatures at the appropriate seasons. The particular birds and animals exhibited are the willow-grouse, the weasel, and a large species of hare. All of these, in their summer garb, have a brown color, which harmonizes marvellously with their surroundings, while in winter they are pure white, to match the snow that for some months covers the ground in their habitat.

The other cases of this interesting exhibit show a large variety of birds and animals under conditions of somewhat abnormal variation, in the one case of albinism and the other of melanism. These cases are, for the casual visitor, perhaps the most striking of all, although, of course, they teach no such comprehensive lessons as the other exhibits just referred to.

The second of the novel exhibits of the museum to which I wish to refer is to be found in a series of alcoves close beside the central cases in the main hallway.

Each of these alcoves is devoted to a class of animals—one to mammals, one to birds, one to fishes, and so on. In each case very beautiful sets of specimens have been prepared, illustrating the anatomy and physiology of the group of animals in question. Here one may see, for example, in the alcove devoted to birds, specimens showing not only details of the skeleton and muscular system, but the more striking examples of variation of form of such members as the bill, legs, wings, and tails. Here are preparations also illustrating, very strikingly, the vocal apparatus of birds. Here, again, are finely prepared wings, in which the various sets of feathers have been outlined with different-colored pigments, so that the student can name them at a glance. In fact, every essential feature of the anatomy of the bird may be studied here as in no other collection that I know of. And the same is true of each of the other grand divisions of the animal kingdom. This exhibit alone gives an opportunity for the student of natural history that is invaluable. It is quite clear to any one who has seen it that every natural history museum must prepare a similar educational exhibit before it can claim to do full justice to its patrons.

A third feature that cannot be overlooked is shown in the numerous cases of stuffed birds, in which the specimens are exhibited, not merely by themselves on conventional perches, but amid natural surroundings, usually associated with their nests and eggs or young. These exhibits have high artistic value in addition to their striking scientific worth. They teach ornithology as it should be taught, giving such clews to the recognition of birds in the fields as are not at all to be found

in ordinary collections of stuffed specimens. This feature of the museum has, to be sure, been imitated in the American Museum of Natural History in New York, but the South Kensington Museum was the first in the field and is still the leader.

PUBLIC INTEREST IN THE MUSEUM

A few words should be added as to the use made by the public of the treasures offered for their free inspection by the British Museum. I shall attempt nothing further than a few data regarding actual visits to the museum. In the year 1899 the total number of such visits aggregated 663,724; in 1900 the figures rise to 689,249—well towards three-quarters of a million. The number of visits is smallest in the winter months, but mounts rapidly in April and May; it recedes slightly for June and July, and then comes forward to full tide in August, during which month more than ninety-five thousand people visited the museum in 1901, the largest attendance in a single day being more than nine thousand. August, of course, is the month of tourists —particularly of tourists from America—but it is interesting and suggestive to note that it is not the tourist alone who visits the British Museum, for the flood-tide days of attendance are always the Bank holidays, including Christmas boxing-day and Easter Monday, when the working-people turn out *en masse*. On these days the number of visits sometimes mounts above ten thousand.

All this, it will be understood, refers exclusively to the main building of the museum on Great Russell Street. But, meantime, out in Kensington, at the nat-

ural history museum, more than half a million visits each year are also made. In the aggregate, then, about a million and a quarter of visits are paid to the British Museum yearly, and though the bulk of the visitors may be mere sight-seers, yet even these must carry away many ideas of value, and it hardly requires argument to show that, as a whole, the educational influence of the British Museum must be enormous. Of its more direct stimulus to scientific work through the trained experts connected with the institution I shall perhaps speak in another connection.

II

THE ROYAL SOCIETY OF LONDON FOR IMPROVING NATURAL KNOWLEDGE

A SESSION OF THE SOCIETY

THERE is one scientific institution in London more venerable and more famous even than the British Museum. This, of course, is the Royal Society, a world-famous body, whose charter dates from 1662, but whose actual sessions began at Gresham College some twenty years earlier. One can best gain a present-day idea of this famous institution by attending one of its weekly meetings in Burlington House, Piccadilly—a great, castle-like structure, which serves also as the abode of the Royal Chemical Society and the Royal Academy of Arts. The formality of an invitation from a fellow is required, but this is easily secured by any scientific visitor who may desire to attend the meeting. The programme of the meeting each week appears in that other great British institution, the *Times*, on Tuesdays.

The weekly meeting itself is held on Thursday afternoon at half-past four. As one enters the door leading off the great court of Burlington House a liveried attendant motions one to the rack where great-coat and hat may be left, and without further ceremony one steps into the reception-room unannounced. It is a middle-sized,

almost square room, pillared and formal in itself, and almost without furniture, save for a long temporary table on one side, over which cups of tea are being handed out to the guests, who cluster there to receive it, and then scatter about the room to sip it at their leisure. We had come to hear a lecture and had expected to be ushered into an auditorium; but we had quite forgotten that this is the hour when all England takes its tea, the *élite* of the scientific world, seemingly, quite as much as the devotees of another kind of society. Indeed, had we come unawares into this room we should never have suspected that we had about us other than an ordinary group of cultured people gathered at a conventional "tea," except, indeed, that suspicion might be aroused by the great preponderance of men—there being only three or four women present—and by the fact that here and there a guest appears in unconventional dress—a short coat or even a velvet working-jacket. For the rest there is the same gathering into clusters of three or four, the same inarticulate clatter of many voices that mark the most commonplace of gatherings.

But if one will withdraw to an inoffensive corner and take a critical view of the assembly, he will presently discover that many of the faces are familiar to him, although he supposed himself to be quite among strangers. The tall figure, with the beautiful, kindly face set in white hair and beard, has surely sat for the familiar portrait of Alfred Russel Wallace. This short, thick-set, robust, business-like figure is that of Sir Norman Lockyer. Yonder frail-seeming scholar, with white beard, is surely Professor Crookes. And this other

scholar, with tall, rather angular frame and most kindly gleam of eye, is Sir Michael Foster; and there beyond is the large-seeming though not tall figure, and the round, rosy, youthful-seeming, beautifully benevolent face of Lord Lister. "What! a real lord there?" said a little American girl to whom I enumerated the company after my first visit to the Royal Society. "Then how did he act? Was he very proud and haughty, as if he could not speak to other people?" And I was happy to be able to reply that though Lord Lister, perhaps of all men living, would be most excusable did he carry in his manner the sense of his achievements and honors, yet in point of fact no man could conceivably be more free from any apparent self-consciousness. As one watches him now he is seen to pass from group to group with cordial hand-shake and pleasant word, clearly the most affable of men, lord though he be, and president of the Royal Society, and foremost scientist of his time.

Presently an attendant passed through the tea-room bearing a tremendous silver mace, perhaps five feet long, surmounted by a massive crown and cross, and looking like nothing so much as a "gigantic war-club." This is the mace which, when deposited on the president's desk in the lecture-room beyond, will signify that the society is in session. "It is the veritable mace," some one whispers at your elbow, "concerning which Cromwell gave his classical command to 'Remove that bauble.'" But since the mace was not made until 1663, some five years after Cromwell's death, this account may lack scientific accuracy. Be that as it may, this mace has held its own far more

ASSEMBLY-ROOM OF THE ROYAL SOCIETY

steadily than the fame of its alleged detractor, and its transportation through the tea-room is the only manner of announcement that the lecture is about to open in the hall beyond. Indeed, so inconspicuous is the proceeding, and so quietly do the members that choose to attend pass into the lecture-hall, leaving perhaps half the company engaged as before, that the "stranger"—as the non-member is here officially designated—might very readily fail to understand that the séance proper had begun. In any event, he cannot enter until permission has been formally voted by the society.

When he is allowed to enter he finds the meeting-room little different from the one he has left, except that it is provided with a sort of throne on a raised platform at one end and with cushioned benches for seats. On the throne, if one may so term it, sits Lord Lister, scarcely more than his head showing above what seems to be a great velvet cushion which surmounts his desk, at the base of which, in full view of the society, rests the mace, fixing the eye of the "stranger," as it is alleged to have fixed that of Cromwell aforetime, with a peculiar fascination. On a lower plane than the president, at his right and left, sit Sir Michael Foster and Professor Arthur William Rücker, the two permanent secretaries. At Sir Michael's right, and one stage nearer the audience, stands the lecturer, on the raised platform and behind the desk which extends clear across the front of the room. As it chances, the lecturer this afternoon is Professor Ehrlich, of Berlin and Frankfort-on-the-Main, who has been invited to deliver the Croonian lecture. He is speaking in German, and hence most of the fellows are assisting

their ears by following the lecture in a printed translation, copies of which, in proof, were to be secured at the door.

The subject of the lecture is "Artificial Immunization from Disease." It is clear that the reader is followed with interested attention, which now and again gives rise to a subdued shuffle of applause.

The fact that the lecturer is speaking German serves perhaps to suggest even more vividly than might otherwise occur to one the contrast between this meeting and a meeting of the corresponding German society—the Royal Academy of Sciences at Berlin. Each is held in an old building of palatial cast and dimensions, of which Burlington House, here in Piccadilly, is much the older—dating from 1664—although its steam-heating and electric-lighting apparatus, when contrasted with the tile stoves and candles of the other, would not suggest this. For the rest, the rooms are not very dissimilar in general appearance, except for the platform and throne. But there the members of the society are shut off from the audience both by the physical barrier of the table and by the striking effect of their appearance in full dress, while here the fellows chiefly compose the audience, there being only a small company of "strangers" present, and these in no way to be distinguished by dress or location from the fellows themselves. It may be added that the custom of the French Academy of Sciences is intermediate between these two. There the visitors occupy seats apart, at the side of the beautiful hall, the main floor being reserved for members. But the members themselves are not otherwise distinguishable, and they

come and go and converse together even during the reading of a paper almost as if this were a mere social gathering. As it is thus the least formal, the French meeting is also by far the most democratic of great scientific gatherings. Its doors are open to whoever may choose to enter. The number who avail themselves of this privilege is not large, but it includes, on occasions, men of varied social status and of diverse races and colors—none of whom, so far as I could ever discern, attracts the slightest attention.

At the German meeting, again, absolute silence reigns. No one thinks of leaving during the session, and to make any sound above a sigh would seem almost a sacrilege. But at the Royal Society an occasional auditor goes or comes, there are repeated audible signs of appreciation of the speaker's words, and at the close of the discourse there is vigorous and prolonged applause. There is also a debate, of the usual character, announced by the president, in which "strangers" are invited to participate, and to which the lecturer finally responds with a brief *Nachwort*, all of which is quite anomalous from the German or French stand-points. After that, however, the meeting is declared adjourned with as little formality in one case as in the others, and the fellows file leisurely out, while the attendant speedily removes the mace, in official token that the séance of the Royal Society is over.

THE LIBRARY AND READING-ROOM

But the "stranger" must not leave the building without mounting to the upper floor for an inspection of the library and reading-room. The rooms

below were rather bare and inornate, contrasting unfavorably with the elegant meeting-room of the French institute. But this library makes full amends for anything that the other rooms may lack. It is one of the most charming—"enchanting" is the word that the Princess Christian is said to have used when she visited it recently—and perhaps quite the most inspiring room to be found in all London. It is not very large as library rooms go, but high, and with a balcony supported by Corinthian columns. The alcoves below are conventional enough, and the high tables down the centre, strewn with scientific periodicals in engaging disorder, are equally conventional. But the color-scheme of the decorations—sage-green and tawny—is harmonious and pleasing, and the effect of the whole is most reposeful and altogether delightful.

Chief distinction is given the room, however, by a row of busts on either side and by certain pieces of apparatus on the centre tables.

The busts, as will readily be surmised, are portraits of distinguished fellows of the Royal Society. There is, however, one exception to this, for one bust is that of a woman—Mary Somerville, translator of the *Mécanique Céleste*, and perhaps the most popular of the scientific writers of her time. It is almost superfluous to state that the row of busts begins with that of Newton. The place of honor opposite is held by that of Faraday. Encircling the room to join these two one sees, among others, the familiar visages of Dr. Gilbert; of Sir Joseph Banks, the famous surgeon of the early nineteenth century, who had the honor of being the only man that ever held the presidential chair of

LIBRARY OF THE ROYAL SOCIETY

the Royal Society longer than it was held by Newton; of James Watts, of "steam-engine" fame; of Sabine, the astronomer, also a president of the society; and of Dr. Falconer and Sir Charles Lyell, the famous geologists.

There are numerous other busts in other rooms, some of them stowed away in nooks and crannies, and the list of those selected for the library does not, perhaps, suggest that this is the room of honor, unless, indeed, the presence of Newton and Faraday gives it that stamp. But in the presence of the images of these two, and of Lyell, to go no farther, one feels a certain sacredness in the surroundings.

If this is true of the mere marble images, what shall we say of the emblems on the centre table? That little tubular affair, mounted on a globe, the whole cased in a glass frame perhaps two feet high, is the first reflecting telescope ever made, and it was shaped by the hand of Isaac Newton. The brass mechanism at the end of the next table is the perfected air-pump of Robert Boyle, Newton's contemporary, one of the founders of the Royal Society and one of the most acute scientific minds of any time. And here between these two mementos is a higher apparatus, with crank and wheel and a large glass bulb that make it conspicuous. This is the electrical machine of Joseph Priestley. There are other mementos of Newton—a stone graven with a sun-dial, which he carved as a boy, on the paternal manor-house; a chair, said to have been his, guarded here by a silk cord against profanation; bits of the famous apple-tree which, as tradition will have it, aided so tangibly in the greatest of discoveries; and the man-

uscript of the *Principia* itself—done by the hand of an amanuensis, to be sure, but with interlinear corrections in the small, clear script of the master-hand itself. Here, too, is the famous death-mask, so much more interesting than any sculptured portrait, and differing so strangely in its broad-based nose and full, firm mouth from the over-refined lineaments of the sculptured bust close at hand. In a room not far away, to reach which one passes a score or two of portraits and as many busts of celebrities —including, by-the-bye, both bust and portrait of Benjamin Franklin—one finds a cabinet containing other mementos similar to those on the library tables. Here is the first model of Davy's safety-lamp; there a chronometer which aided Cook in his famous voyage round the world. This is Wollaston's celebrated "Thimble Battery." It will slip readily into the pocket, yet he jestingly showed it to a visitor as "his entire laboratory." That is a model of the double-decked boat made by Sir William Petty, and there beyond is a specimen of almost, if not quite, the first radiometer devised by Sir William Crookes.

As one stands in the presence of all these priceless relics, so vividly do the traditions of more than two centuries of science come to mind that one seems almost to have lived through them. One recalls, as if it were a personal recollection, the founding of the Royal Society itself in 1662, and the extraordinary scenes which the society witnessed during the years of its adolescence.

As one views the mementos of Boyle and Newton, one seems to be living in the close of the seventeenth

century. It is a troublous time in England. Revolution has followed revolution. Commonwealth has supplanted monarchy and monarchy commonwealth. At last the "glorious revolution" of 1688 has placed a secure monarch on the throne. But now one external war follows another, and the new king, William of Orange, is leading the "Grand Alliance" against the French despot Louis XIV. There is war everywhere in Europe, and the treaty of Ryswick, in 1697, is but the preparation for the war of the Spanish Alliance, which will usher in the new century. But amid all this political turmoil the march of scientific discovery has gone serenely on; or, if not serenely, then steadily, and perhaps as serenely as could be hoped. Boyle has discovered the law of the elasticity of gases and a host of minor things. Robert Hooke is on the track of many marvels. But all else pales before the fact that Newton has just given to the world his marvellous law of gravitation, which has been published, with authority of the Royal Society, through the financial aid of Halley. The brilliant but erratic Hooke has contested the priority of discovery and strenuously claimed a share in it. Halley eventually urges Newton to consider Hooke's claim in some of the details, and Newton yields to the extent of admitting that the great fact of gravitational force varying inversely as the square of the distance had been independently discovered by Hooke; but he includes also Halley himself and Sir Christopher Wren, along with Hooke, as equally independent discoverers of the same principle. To the twentieth-century consciousness it seems odd to hear Wren thus named as a scientific discoverer; but

in truth the builder of St. Paul's began life as a professor of astronomy at Gresham College, and was the immediate predecessor of Newton himself in the presidential chair of the Royal Society. Now, at the very close of the seventeenth century, Boyle is recently dead, but Hooke, Wren, Halley, and Newton still survive: some of them are scarcely past their prime. It is a wonderful galaxy of stars of the first magnitude, and even should no other such names come in after-time, England's place among the scientific constellations is secure.

But now as we turn to the souvenirs of Cooke and Wollaston and Davy the scene shifts by a hundred years. We are standing now in the closing epoch of the eighteenth century. These again are troublous times. The great new colony in the West has just broken off from the parent swarm. Now all Europe is in turmoil. The French war-cloud casts its ominous shadow everywhere. Even in England mutterings of the French Revolution are not without an echo. The spirit of war is in the air. And yet, as before, the spirit of science also is in the air. The strain of the political relations does not prevent a perpetual exchange of courtesy between scientific men and scientific bodies of various nations. Davy's dictum that "science knows no country" is perpetually exemplified in practice. And at the Royal Society, to match the great figures that were upon the scene a century before, there are such men as the eccentric Cavendish, the profound Wollaston, the marvellously versatile Priestley, and the equally versatile and even keener-visioned Rumford. Here, too, are Herschel, who is

giving the world a marvellous insight into the constitution of the universe; and Hutton, who for the first time gains a clear view of the architecture of our earth's crust; and Jenner, who is rescuing his fellow-men from the clutches of the most deadly of plagues; to say nothing of such titanic striplings as Young and Davy, who are just entering the scientific lists. With such a company about us we are surely justified in feeling that the glory of England as a scientific centre has not dimmed in these first hundred and thirty years of the Royal Society's existence.

And now, as we view the radiometer, the scene shifts by yet another century, and we come out of cloud-land and into our own proper age. We are at the close of the nineteenth century—no, I forget, we are fairly entering upon the twentieth. Need I say that these again are troublous times? Man still wages warfare on his fellow-man as he has done time out of mind; as he will do—who shall say how long? But meantime, as of yore, the men of science have kept steadily on their course. But recently here at the Royal Society were seen the familiar figures of Darwin and Lyell and Huxley and Tyndall. Nor need we shun any comparison with the past while the present lists can show such names as Wallace, Kelvin, Lister, Crookes, Foster, Evans, Rayleigh, Ramsay, and Lockyer. What revolutionary advances these names connote! How little did those great men of the closing decades of the seventeenth and eighteenth centuries know of the momentous truths of organic evolution for which the names of Darwin and Wallace and Huxley stand! How little did they know a century ago,

despite Hutton's clear prevision, of these marvellous slow revolutions through which, as Lyell taught us, the earth's crust had been built up! Not even Jenner could foresee a century ago the revolution in surgery which has been effected in our generation through the teachings of Lister.

And what did Rumford and Davy know of energy in its various manifestations as compared with the knowledge of to-day, of Crookes and Rayleigh and Ramsay and Kelvin? What would Joseph Priestley, the discoverer of oxygen, and Cavendish, the discoverer of nitrogen, think could they step into the laboratory of Professor Ramsay and see test-tubes containing argon and helium and krypton and neon and zenon? Could they more than vaguely understand the papers contributed in recent years to the Royal Society, in which Professor Ramsay explains how these new constituents of the atmosphere are obtained by experiments on liquid air. "Here," says Professor Ramsay, in effect, in a late paper to the society, "is the apparatus with which we liquefy hydrogen in order to separate neon from helium by liquefying the former while the helium still remains gaseous." Neon, helium, liquid air, liquid hydrogen—these would seem strange terms to the men who on discovering oxygen and nitrogen named them "dephlogisticated air" and "phlogisticated air" respectively.

Again, how elementary seems the teaching of Herschel, wonderful though it was in its day, when compared with our present knowledge of the sidereal system as outlined in the theories of Sir Norman Lockyer. Herschel studied the sun-spots, for example, with

assiduity, and even suggested a possible connection between sun-spots and terrestrial weather. So far, then, he would not be surprised on hearing the announcement of Professor Lockyer's recent paper before the Royal Society on the connection between sun-spots and the rainfall in India. But when the paper goes on to speak of the actual chemical nature of the sun-spots, as tested by a spectroscope; to tell of a "cool" stage when the vapor of iron furnishes chief spectrum lines, and of a "hot" stage when the iron has presumably been dissociated into unknown "proto-iron" constituents—then indeed does it go far beyond the comprehension of the keenest eighteenth-century intellect, though keeping within the range of understanding of the mere scientific tyro of to-day.

Or yet again, consider a recent paper contributed by Professor Lockyer to the Royal Society, entitled "The New Star in Perseus: Preliminary Note"—referring to the new star that flashed suddenly on the vision of the terrestrial observers at more than first magnitude on February 22, 1901. This "star," the paper tells us, when studied by its spectrum, is seen to be due to the impact of two swarms of meteors out in space—swarms moving in different directions "with a differential velocity of something like seven hundred miles a second." Every astronomer of to-day understands how such a record is read from the displacement of lines on the spectrum, as recorded on the photographic negative. But imagine Sir William Herschel, roused from a century's slumber, listening to this paper, which involves a subject of which he was the first great master. "Nebulæ," he might say; "yes, they were a specialty of

mine; but swarms of meteors — I know nothing of these. And 'spectroscopes,' 'photographs' — what, pray, are these? In my day there were no such words or things as spectroscope and photograph; to my mind these words convey no meaning."

But why go farther? These imaginings suffice to point a moral that he who runs may read. Of a truth the march of science still goes on as it has gone on with steady tread throughout the long generations of the Royal Society's existence. If the society had giants among its members in the days of its childhood and adolescence, no less are there giants still to keep up its fame in the time of its maturity. The place of England among the scientific constellations is secure through tradition, but not through tradition alone.

III

THE ROYAL INSTITUTION AND THE LOW-TEMPERATURE RESEARCHES

FOUNDATION AND FOUNDER

"GEORGE THE THIRD, by the Grace of God King of Great Britain, France, and Ireland, Defender of the Faith, etc., to all to whom these presents shall come, greeting. Whereas several of our loving subjects are desirous of forming a Public Institution for diffusing the knowledge and facilitating the general introduction of Useful Mechanical Inventions and Improvements; and for teaching, by Courses of Philosophical Lectures and Experiments, the Application of Science to the Common Purposes of Life, we do hereby give and grant"—multifarious things which need not here be quoted. Such are the opening words of the charter with which, a little more than a century ago, the Royal Institution of Great Britain came into existence and received its legal christening. If one reads on he finds that the things thus graciously "given and granted," despite all the official verbiage, amount to nothing more than royal sanction and approval, but doubtless that meant more in the way of assuring popular approval than might at first glimpse appear. So, too, of the list of earls, baronets, and the like, who appear as officers and managers of the undertaking, and who are described in the charter as "our right trusty and

right well-beloved cousins," "our right trusty and well-beloved counsellors," and so on, in the skilfully graduated language of diplomacy. The institution that had the King for patron and such notables for officers seemed assured a bright career from the very beginning. In name and in personnel it had the flavor of aristocracy, a flavor that never palls on British palate. And right well the institution has fulfilled its promise, though in a far different way from what its originator and founder anticipated.

Its originator and founder, I say, and say advisedly; for, of course, here, as always, there is one man who is the true heart and soul of the movement, one name that stands, in truth, for the whole project, and to which all the other names are mere appendages. You would never suspect which name it is, in the present case, from a study of the charter, for it appears well down the file of graded titles, after "cousins" and "counsellors" have had their day, and is noted simply as "our trusty and well-beloved Benjamin, Count of Rumford, of the Holy Roman Empire." Little as there is to signalize it in the charter, this is the name of the sole projector of the enterprise in its incipiency, of the projector of every detail, of the writer of the charter itself even. The establishment thus launched with royal title might with full propriety have been called, as indeed it sometimes is called, the Rumford Institution.

The man who thus became the founder of this remarkable institution was in many ways a most extraordinary person. He was an American by birth, and if not the most remarkable of Americans, he surely

was destined to a more picturesque career than ever fell to the lot of any of his countrymen of like eminence. Born on a Massachusetts farm, he was a typical "down-east Yankee," with genius added to the usual shrewd, inquiring mind and native resourcefulness. He was self-educated and self-made in the fullest sense in which those terms can be applied. At fourteen he was an unschooled grocer-lad—Benjamin Thompson by name—in a little New England village; at forty he was a world-famous savant, as facile with French, Italian, Spanish, and German as with his native tongue; he had become vice-president and medallist of the Royal Society, member of the Berlin National Academy of Science, of the French Institute, of the American Academy of Science, and I know not what other learned bodies; he had been knighted in Great Britain after serving there as under-secretary of state and as an officer; and he had risen in Bavaria to be more than half a king in power, with the titles, among others, of privy councillor of state, and head of the war department, lieutenant-general of the Bavarian armies, holder of the Polish order of St. Stanislas and the Bavarian order of the White Eagle, ambassador to England and to France, and, finally, count of the Holy Roman Empire. Once, in a time of crisis, Rumford was actually left at the head of a council of regency, in full charge of Bavarian affairs, the elector having fled. The Yankee grocer-boy had become more than half a king.

Never, perhaps, did a man of equal scientific attainments enjoy a corresponding political power. Never was political power wielded more justly by any man.

For in the midst of all his political and military triumphs, Rumford remained at heart to the very end the scientist and humanitarian. He wielded power for the good of mankind; he was not merely a ruler but a public educator. He taught the people of Bavaria economy and Yankee thrift. He established kitchens for feeding the poor on a plan that was adopted all over Europe; but, better yet, he created also workshops for their employment and pleasure-gardens for their recreation. He actually banished beggary from the principality.

It was in the hope of doing in some measure for London what he had done for Munich that this large-brained and large-hearted man was led to the project of the Royal Institution. He first discussed his plans with a committee of the Society for Alleviating the Condition of the Poor, for it was the poor, the lower ranks of society, whom he wished chiefly to benefit. But he knew that to accomplish his object, he must work through the aristocratic channels; hence the name of the establishment and the charter with its list of notables. The word institution was selected by Rumford, after much deliberation, as, on the whole, the least objectionable title for the establishment, as having a general inclusiveness not possessed by such words as school or college. Yet in effect it was a school which Rumford intended to found—a school for the general diffusion of useful knowledge. There were to be classes for mechanics, and workshops, kitchens, and model-rooms, where the "application of science to the useful purposes of life" might be directly and practically taught; also a laboratory for more tech-

ENTRANCE TO THE ROYAL INSTITUTION, LONDON

nical investigations, with a "professor" in charge, who should also deliver popular lectures on science. Finally, there was to be a scientific library.

All these aims were put into effect almost from the beginning. The necessary funds were supplied solely by popular subscription and by the sale of lecture tickets (as all funds of the institution have been ever since), and before the close of the year 1800 Rumford's dream had become an actuality—as this practical man's dreams nearly always did. The new machine did not move altogether without friction, of course, but on the whole all went well for the first few years. The institution had found a local habitation in a large building in Albemarle Street, the same building which it still occupies, and for a time Rumford lived there and gave the enterprise his undivided attention. He appointed the brilliant young Humphry Davy to the professorship of chemistry, and the even more wonderful Thomas Young to that of natural philosophy. He saw the workshops and kitchens and model-rooms in running order—the entire enterprise fully launched. Then other affairs, particularly an attachment for a French lady, the widow of the famous chemist Lavoisier (whom he subsequently married, to his sorrow), called him away from England never to return. And the first chapter in the history of the Royal Institution was finished.

METHOD AND RESULT

Rumford, the humanitarian, gone, a curious change came over the spirit of the enterprise he had founded. The aristocrats who at first were merely ballast for the

enterprise now made their influence felt. With true British reserve, they announced their belief that the education of the masses involved a dangerous political tendency. Hence the mechanics' school was suspended and the workshops and kitchens abolished; in a word, the chief ends for which the institution was founded were annulled. The library and the lectures remained, to be sure, but they were for the amusement of the rich, not for the betterment of the poor. It was the West End that made a fad of the institution and a society function of the lectures of Sydney Smith and of the charming youth Davy. Thus the institution came to justify its aristocratic title and its regal patronage; and the poor seemed quite forgotten.

But indeed the institution itself was poor enough in these days, after the first flush of enthusiasm died away, and it is but fair to remember that without the support of its popular lectures its very existence would have been threatened. Nor in any event are regrets much in order over the possible might-have-beens of an institution whose laboratories were the seat of the physical investigations of Thomas Young, through which the wave theory of light first gained a footing, and of the brilliant chemical researches of Davy, which practically founded the science of electro-chemistry and gave the chemical world first knowledge of a galaxy of hitherto unknown elements. Through the labors of these men, and through the popular lecture-courses delivered at the institution by such other notables of science as Wollaston, Dalton, and Rumford, the enterprise had become world-famous before the close of the first decade of its existence.

From that day till this the character of the Royal Institution has not greatly changed. The enterprise shifted around during its earliest years, while it was gaining its place in the scheme of things; but once that was found, like a true British institution it held its course with an inertia that a mere century of time could not be expected to alter. Rumford was the sole founder of the enterprise, but it was Davy who gave it the final and definitive cast. He it was who established the tradition that the Royal Institution was to be essentially a laboratory for brilliant original investigations, the investigator to deliver a yearly course of lectures, but to be otherwise untrammelled. It occupied, and has continued to occupy, the anomalous position of a school to which pupils are on no account admitted, and whose professors teach nothing except by a brief course of lectures to which whoever cares to pay the admission price may freely enter.

But the marvellous results achieved at the Royal Institution have more than justified the existence of so anomalous an enterprise. Superlatives are always dangerous, but it may well be doubted whether there is another single institution in the world where so many novel original discoveries in physical science have been made as have been brought to light in the laboratories of the building on Albemarle Street during this first century of its occupancy; for practically all that is to be credited to Thomas Young, Humphry Davy, Michael Faraday, and John Tyndall, not to mention living investigators, is to be credited also to the Royal Institution, whose professorial chairs these great men have successively occupied. Davy spent here the

best years of his youth and prime. Faraday, his direct successor, came to the institution in a subordinate capacity as a mere boy, and was the life of the institution for half a century. Tyndall gave it forty years of service. What wonder, then, that the Briton speaks of the institution as the "Pantheon of Science"?

If you visit the Royal Institution to-day you will find it in most exterior respects not unlike what it presumably was a century ago. Its long, stone front, dinged with age, with its somewhat Pantheon-like colonnade, has an appearance of dignity rather than of striking impressiveness. The main entrance, jutting full on the sidewalk, is at the street level, and the glass door gives hospitable glimpses of the interior. Entering, one finds himself in a main central hall, at the foot of the main central staircase. The air of eminent respectability so characteristic of the British institution is over all; likewise the pervasive hush of British reserve. But you will not miss also the atmosphere of sincere if uneffusive British courtesy.

At your right, as you mount the stairway, is a large statue of Faraday; on the wall right ahead is a bronze medallion of Tyndall, placed beneath a large portrait of Davy. At the turn of the stairs is a marble bust of Wollaston. Farther on, in hall and library, you will find other busts of Faraday, other portraits of Davy; portraits of Faraday everywhere, and various other busts of notables who have had connection with the institution. You will be shown the lecture-hall where Davy, Faraday, and Tyndall pronounced their marvellous discourses; the arrangement, the seats, the cushions even if appearances speak truly, and certain-

ly the lecture-desk itself, unchanged within the century. You may see the crude balance, clumsy indeed to modern eyes, with which Davy performed his wonders. The names and the memories of three great men—Davy, Faraday, and Tyndall—will be incessantly before you, and the least impressionable person could not well escape a certain sense of consecration of his surroundings. The hush that is over everything seems but fitting.

All that is as it should be. But there are other memories connected with these surroundings which are not so tangibly presented to the senses. For where, amid all these busts and portraits, is the image of that other great man, the founder of the institution, the sole originator of the enterprise which has made possible the aggregation of all these names and these memories? Where are the remembrances of that extraordinary man whom the original charter describes as "our well-beloved Benjamin, Count of Rumford?" Well, you will find a portrait of him, it is true, if you search far enough, hung high above a doorway in a room with other portraits. But one finds it hard to escape the feeling that there has been just a trifling miscarriage of justice in the disposal. Doubtless there was no such intention, but the truth seems to be that the glamour of the newer fame of Faraday has dazzled a little the eyes of the rulers of the institution of the present generation. But that, after all, is a small matter about which to quibble. There is glory enough for all in the Royal Institution, and the disposal of busts and portraits is unworthy to be mentioned in connection with the lasting fame of the great men

who are here in question. It would matter little if there were no portrait at all of Rumford here, for all the world knows that the Royal Institution itself is in effect his monument. His name will always be linked in scientific annals with the names of Young, Davy, Faraday, and Tyndall. And it is worthy such association, for neither in native genius nor in realized accomplishments was Rumford inferior to these successors.

FROM LIQUID CHLORINE TO LIQUID HYDROGEN

Nor is it merely by mutual association with the history of the Royal Institution that these great names are linked. There was a curious and even more lasting bond between them in the character of their scientific discoveries. They were all pioneers in the study of those manifestations of molecular activity which we now, following Young himself, term energy. Thus Rumford, Davy, and Young stood almost alone among the prominent scientists of the world at the beginning of the century in upholding the idea that heat is not a material substance—a chemical element—but merely a manifestation of the activities of particles of matter. Rumford's papers on this thesis, communicated to the Royal Society, were almost the first widely heralded claims for this then novel idea. Then Davy came forward in support of Rumford, with his famous experiment of melting ice by friction. It was perhaps this intellectual affinity that led Rumford to select Davy for the professorship at the Royal Institution, and thus in a sense to predetermine the character of the scientific work that should be accomplished there

—the impulse which Davy himself received from Rumford being passed on to his pupil Faraday. There is, then, an intangible but none the less potent web of association between the scientific work of Rumford and some of the most important researches that were conducted at the Royal Institution long years after his death; and one is led to feel that it was not merely a coincidence that some of Faraday's most important labors should have served to place on a firm footing the thesis for which Rumford battled; and that Tyndall should have been the first in his "beautiful book" called *Heat, a Mode of Motion*, to give wide popular announcement to the fact that at last the scientific world had accepted the proposition which Rumford had vainly demonstrated three-quarters of a century before.

This same web of association extends just as clearly to the most important work which has been done at the Royal Institution in the present generation, and which is still being prosecuted there—the work, namely, of Professor James Dewar on the properties of matter at excessively low temperatures. Indeed, this work is in the clearest sense a direct continuation of researches which Davy and Faraday inaugurated in 1823 and which Faraday continued in 1844. In the former year Faraday, acting on a suggestion of Davy's, performed an experiment which resulted in the production of a "clear yellow oil" which was presently proved to be liquid chlorine. Now chlorine, in its pure state, had previously been known (except in a forgotten experiment of Northmore's) only as a gas. Its transmutation into liquid form was therefore regarded as a very

startling phenomenon. But the clew thus gained, other gases were subjected to similar conditions by Davy, and particularly by Faraday, with the result that several of them, including sulphurous, carbonic, and hydrochloric acids were liquefied. The method employed, stated in familiar terms, was the application of cold and of pressure. The results went far towards justifying an extraordinary prediction made by that extraordinary man, John Dalton, as long ago as 1801, to the effect that by sufficient cooling and compressing all gases might be transformed into liquids —a conclusion to which Dalton had vaulted, with the sureness of supreme genius, from his famous studies of the properties of aqueous vapor.

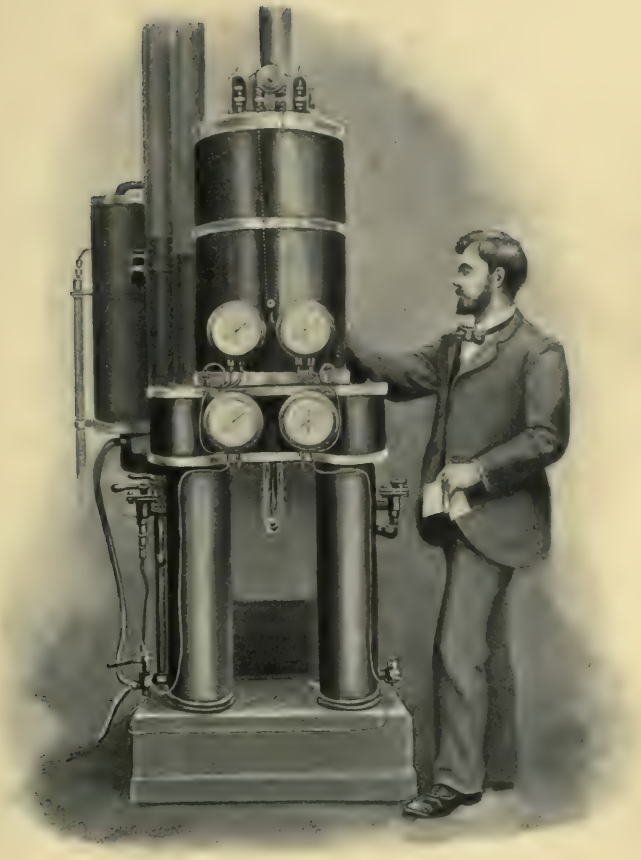
Between Dalton's theoretical conclusion, however, and experimental demonstration there was a tremendous gap, which the means at the disposal of the scientific world in 1823 did not enable Davy and Faraday more than partially to bridge. A long list of gases, including the familiar oxygen, hydrogen, and nitrogen, resisted all their efforts utterly—notwithstanding the facility with which hydrogen and oxygen are liquefied when combined in the form of water-vapor, and the relative ease with which nitrogen and hydrogen, combined to form ammonia, could also be liquefied. Davy and Faraday were well satisfied of the truth of Dalton's proposition, but they saw the futility of further efforts to put it into effect until new means of producing, on the one hand, greater pressures, and, on the other, more extreme degrees of cold, should be practically available. So the experiments of 1823 were abandoned.

But in 1844 Faraday returned to them, armed now with new weapons, in the way of better air-pumps and colder freezing mixtures, which the labors of other workers, chiefly Thilorier, Mitchell, and Natterer, had made available. With these new means, and without the application of any principle other than the use of cold and pressure as before, Faraday now succeeded in reducing to the liquid form all the gases then known with the exception of six; while a large number of these substances were still further reduced, by the application of the extreme degrees of cold now attained, to the condition of solids. The six gases which still proved intractable, and which hence came to be spoken of as "permanent gases," were nitrous oxide, marsh gas, carbonic oxide, oxygen, nitrogen, and hydrogen.

These six refractory gases now became a target for the experiments of a host of workers in all parts of the world. The resources of mechanical ingenuity of the time were exhausted in the effort to produce low temperatures on the one hand and high pressures on the other. Thus Andrews, in England, using the bath of solid carbonic acid and ether which Thilorier had discovered, and which produces a degree of cold of $-80°$ Centigrade, applied a pressure of five hundred atmospheres, or nearly four tons to the square inch, without producing any change of state. Natterer increased this pressure to two thousand seven hundred atmospheres, or twenty-one tons to the square inch, with the same negative results. The result of Andrews' experiments in particular was the final proof of what Cagniard de la Tour had early suspected and Faraday had firmly believed, that pressure alone, regardless of

temperature, is not sufficient to reduce a gas to the liquid state. In other words, the fact of a so-called "critical temperature," varying for different substances, above which a given substance is always a gas, regardless of pressure, was definitively discovered. It became clear, then, that before the resistant gases would be liquefied means of reaching extremely low temperatures must be discovered. And for this, what was needed was not so much new principles as elaborate and costly machinery for the application of a principle long familiar—the principle, namely, that an evaporating liquid reduces the temperature of its immediate surroundings, including its own substance.

Ingenious means of applying this principle, in connection with the means previously employed, were developed independently by Pictet in Geneva and Cailletet in Paris, and a little later by the Cracow professors Wroblewski and Olzewski, also working independently. Pictet, working on a commercial scale, employed a series of liquefied gases to gain lower and lower temperatures by successive stages. Evaporating sulphurous acid liquefied carbonic acid, and this in evaporating brought oxygen under pressure to near its liquefaction point; and, the pressure being suddenly released (a method employed in Faraday's earliest experiments), the rapid expansion of the compressed oxygen liquefies a portion of its substance. This result was obtained in 1877 by Pictet and Cailletet almost simultaneously. Cailletet had also liquefied the newly discovered acetylene gas. Five years later Wroblewski liquefied marsh gas, and the following year nitrogen; while carbonic oxide and nitrous oxide yielded



PROFESSOR DEWAR'S APPARATUS WITH WHICH HYDROGEN WAS FIRST LIQUEFIED

to Olzewski in 1884. Thus forty years of effort had been required to conquer five of Faraday's refractory gases, and the sixth, hydrogen, still remains resistant. Hydrogen had, indeed, been seen to assume the form of visible vapor, but it had not been reduced to the so-called static state—that is, the droplets had not been collected in an appreciable quantity, as water is collected in a cup. Until this should be done, the final problem of the liquefaction of hydrogen could not be regarded as satisfactorily solved.

More than another decade was required to make this final step in the completion of Faraday's work. And, oddly enough, yet very fittingly, it was reserved for Faraday's successor in the chair at the Royal Institution to effect this culmination. Since 1884 Professor Dewar's work has made the Royal Institution again the centre of low-temperature research. By means of improved machinery and of ingenious devices for shielding the substance operated on from the accession of heat, to which reference will be made more in detail presently, Professor Dewar was able to liquefy the gas fluorine, recently isolated by Moussan, and the recently discovered gas helium in 1897. And in May, 1898, he was able to announce that hydrogen also had yielded, and for the first time in the history of science that elusive substance, hitherto "permanently" gaseous, was held as a tangible liquid in a cuplike receptacle; and this closing scene of the long struggle was enacted in the same laboratory in which Faraday performed the first liquefaction experiment with chlorine just three-quarters of a century before.

It must be noted, however, that this final stage in

the liquefaction struggle was not effected through the use of the principle of evaporating liquids which has just been referred to, but by the application of a quite different principle and its elaboration into a perfectly novel method. This principle is the one established long ago by Joule and Thomson (Lord Kelvin), that compressed gases when allowed to expand freely are lowered in temperature. In this well-known principle the means was at hand greatly to simplify and improve the method of liquefaction of gases, only for a long time no one recognized the fact. Finally, however, the idea had occurred to two men almost simultaneously and quite independently. One of these was Professor Linde, the well-known German experimenter with refrigeration processes; the other, Dr. William Hampson, a young English physician. Each of these men conceived the idea—and ultimately elaborated it in practice—of accumulating the cooling effect of an expanding gas by allowing the expansion to take place through a small orifice into a chamber in which the coil containing the compressed gas was held. In Dr. Hampson's words:

"The method consists in directing all the gas immediately after its expansion over the coils which contain the compressed gas that is on its way to the expansion-point. The cold developed by expansion in the first expanded gas is thus communicated to the oncoming compressed gas, which consequently expands from, and therefore to, a lower temperature than the preceding portion. It communicates in the same way its own intensified cold to the succeeding portion of

HAMPSON'S APPARATUS FOR LIQUEFYING AIR

compressed gas, which, in its turn, is made colder, both before and after expansion, than any that had gone before. This intensification of cooling goes on until the expansion-temperature is far lower than it was at starting; and if the apparatus be well arranged the effect is so powerful that even the smaller amount of cooling due to the free expansion of gas through a throttle-valve, though pronounced by Siemens and Coleman incapable of being utilized, may be made to liquefy air without using other refrigerants."

So well is this principle carried out in Dr. Hampson's apparatus for liquefying air that compressed air passing into the coil at ordinary temperature without other means of refrigeration begins to liquefy in about six minutes—a result that seems almost miraculous when it is understood that the essential mechanism by which this is brought about is contained in a cylinder only eighteen inches long and seven inches in diameter.

As has been said, it was by adopting this principle of self-intensive refrigeration that Professor Dewar was able to liquefy hydrogen. More recently the same result has been attained through use of the same principle by Professor Ramsay and Dr. Travers at University College, London, who are to be credited also with first publishing a detailed account of the various stages of the process. It appears that the use of the self-intensification principle alone is not sufficient with hydrogen as it is with the less volatile gases, including air, for the reason that at all ordinary temperatures hydrogen does not cool in expanding, but actually becomes warmer. It is only after the compressed hy-

drogen has been cooled by immersion in refrigerating media of very low temperature that this gas becomes amenable to the law of cooling on expansion. In the apparatus used at University College the coil of compressed hydrogen is passed successively through (1) a jar containing alcohol and solid carbonic acid at a temperature of −80° Centigrade; (2) a chamber containing liquid air at atmospheric pressure, and (3) liquid air boiling in a vacuum bringing the temperature to perhaps 205° Centigrade before entering the Hampson coil, in which expansion and the self-intensive refrigeration lead to actual liquefaction. With this apparatus Dr. Travers succeeded in producing an abundant quantity of liquid hydrogen for use in the experiments on the new gases that were first discovered in the same laboratory through the experiments on liquid air—gases about which I shall have something more to say in another chapter.

PRINCIPLES AND EXPERIMENTS

At first blush it seems a very marvellous thing, this liquefaction of substances that under all ordinary conditions are gaseous. It is certainly a little startling to have a cup of clear, water-like liquid offered one, with the assurance that it is nothing but air; still more so to have the same air presented in the form of a white "avalanche snow." In a certain sense it is marvellous, because the mechanical difficulties that have been overcome in reducing the air to these unusual conditions are great. Yet, in another and broader view, there is nothing more wonderful about liquid air than

about liquid water, or liquid mercury, or liquid iron. Long before air was actually liquefied, it was perfectly understood by men of science that under certain conditions it could be liquefied just as surely as water, mercury, iron, and every other substance could be brought to a similar state. This being known, and the principles involved understood, had there been nothing more involved than the bare effort to realize these conditions all the recent low-temperature work would have been mere scientific child's-play, and liquid air would be but a toy of science. But in point of fact there are many other things than this involved; new principles were being searched for and found in the course of the application of the old ones; new light was being thrown into many dark corners; new fields of research, some of them as yet barely entered, were being thrown open to the investigator; new applications of energy, of vast importance not merely in pure science but in commercial life as well, were being made available. That is why the low-temperature work must be regarded as one of the most important scientific accomplishments of our century.

At the very outset it was this work in large measure which gave the final answer to the long-mooted question as to the nature of heat, demonstrating the correctness of Count Rumford's view that heat is only a condition not itself a substance. Since about the middle of the century this view, known as the mechanical theory of heat, has been the constant guide of the physicists in all their experiments, and any one who would understand the low-temperature phenomena must keep this conception of the nature of heat clearly

and constantly in mind. To understand the theory, one must think of all matter as composed of minute isolated particles or molecules, which are always in motion—vibrating, if you will. He must mentally magnify and visualize these particles till he sees them quivering before him, like tuning-forks held in the hand. Remember, then, that, like the tuning-fork, each molecule would, if left to itself, quiver less and less violently, until it ran down altogether, but that the motion thus lessening is not really lost. It is sent out in the form of ether waves, which can set up like motion in any other particles which they reach, be they near or remote; or it is transmitted as a direct push—a kick, if you will—to any other particle with which the molecule comes in physical contact.

But note now, further, that our molecule, while incessantly giving out its energy of motion in ether waves and in direct pushes, is at the same time just as ceaselessly receiving motion from the ether waves made by other atoms, and by the return push of the molecules against which it pushes. In a word, then, every molecule of matter is at once a centre for the distribution of motion (sending out impulses which affect, sooner or later, every other atom of matter in the universe), and, from the other point of view, also a centre for the reception of motion from every direction and from every other particle of matter in the universe. Whether any given molecule will on the whole gain motion or lose it depends clearly on the simple mechanical principles of give and take.

From equally familiar mechanical principles, it is clear that our vibrating molecule, in virtue of its vi-

brations, is elastic, tending to be thrown back from every other molecule with which it comes in contact, just as a vibrating tuning-fork kicks itself away from anything it touches. And of course the vigor of the recoil will depend upon the vigor of the vibration and the previous movements. But since these movements constitute temperature, this is another way of saying that the higher the temperature of a body the more its molecules will tend to spring asunder, such separation in the aggregate constituting expansion of the mass as a whole. Thus the familiar fact of expansion of a body under increased temperature is explained.

But now, since all molecules are vibrating, and so tending to separate, it is clear that no unconfined mass of molecules would long remain in contiguity unless some counter influence tended to draw them together. Such a counter influence in fact exists, and is termed the "force" of cohesion. This force is a veritable gravitation influence, drawing every molecule towards every other molecule. Possibly it is identical with gravitation. It seems subject to some law of decreasing in power with the square of the distance; or, at any rate, it clearly becomes less potent as the distance through which it operates increases.

Now, between this force of cohesion which tends to draw the molecules together, and the heat vibrations which tend to throw the molecules farther asunder, there seems to be an incessant battle. If cohesion prevails, the molecules are held for the time into a relatively fixed system, which we term the solid state. If the two forces about balance each other, the molecules move among themselves more freely but maintain an

average distance, and we term the condition the liquid state. But if the heat impulse preponderates, the molecules (unless restrained from without) fly farther and farther asunder, moving so actively that when they collide the recoil is too great to be checked by cohesion, and this condition we term the gaseous state.

Now after this statement, it is clear that what the low-temperature worker does when he would liquefy a gas is to become the champion of the force of cohesion. He cannot directly aid it, for so far as is known it is an unalterable quantity, like gravitation. But he can accomplish the same thing indirectly by weakening the power of the rival force. Thus, if he encloses a portion of gas in a cylinder and drives a piston down against it, he is virtually aiding cohesion by forcing the molecules closer together, so that the hold of cohesion, acting through a less distance, is stronger. What he accomplishes here is not all gain, however, for the bounding molecules, thus jammed together, come in collision with one another more and more frequently, and thus their average activity of vibration is increased and not diminished; in other words, the temperature of the gas has risen in virtue of the compression. Compression alone, then, will not avail to enable cohesion to win the battle.

But the physicist has another resource. He may place the cylinder of gas in a cold medium, so that the heat vibrations sent into it will be less vigorous than those it sends out. That is a blow the molecule cannot withstand. It is quite impotent to cease sending out the impulses however little comes in return; hence the

aggregate motion becomes less and less active, until finally the molecule is moving so sluggishly that when it collides with its fellow cohesion is able to hold it there. Cohesion, then, has won the battle, and the gas has become a liquid.

Such, stated in terms of the mechanical theory of heat, is what is brought to pass when a gas is liquefied in the laboratory of the physicist. It remains only to note that different chemical substances show the widest diversity as to the exact point of temperature at which this balance of the expansive and cohesive tendencies is affected, but that the point, under uniform conditions of pressure, is always the same for the same substance. This diversity has to do pretty clearly with the size of the individual molecules involved; but its exact explanation is not yet forthcoming, and, except in a general way, the physicist would not be able to predict the "critical temperature" of any new gas presented to him. But once this has been determined by experiment, he always knows just what to expect of any given substance. He knows, for example, that in a mixture of gases hydrogen would still remain gaseous after all the others had assumed the liquid state, and most of them the solid state as well.

These mechanical conceptions well in mind, it is clear that what the would-be liquefier of gases has all along sought to attain is merely the insulation of the portion of matter with which he worked against the access of heat-impulse from its environment. It is clear that were any texture known which would permit a heat-impulse to pass through it in one direction only, nothing more would be necessary than to place a portion of

gas in such a receptacle of this substance, so faced as to permit egress but not entrance of the heat, and the gas thus enclosed, were it hydrogen itself, would very soon become liquid and solid, through spontaneous giving off of its energy, without any manipulation whatever. Contrariwise, were the faces of the receptacle reversed, a piece of iron placed within it would be made red-hot and melted though the receptacle were kept packed in salt and ice and no heat applied except such as came from this freezing mixture. One could cook a beefsteak with a cake of ice had he but such a material as this with which to make his stove. Not even Rumford or our modern Edward Atkinson ever dreamed of such economy of fuel as that.

But, unfortunately, no such substance as this is known, nor, indeed, any substance that will fully prevent the passage of heat-impulses in either direction. Hence one of the greatest tasks of the experimenters has been to find a receptacle that would insulate a cooled substance even partially from the incessant bombardment of heat-impulses from without. It is obvious that unless such an insulating receptacle could be provided none of the more resistent gases, such as oxygen, could be long kept liquid, even when once brought to that condition, since an environment of requisite frigidity could not practicably be provided.

But now another phase of the problem presents itself to the experimenter. Oxygen has assumed the quiescent liquid state, to be sure, but in so doing it has fallen below the temperature of its cooling medium; hence it is now receiving from that medium more energy of vibration than it gives, and unless this is pre-

vented very soon its particles will again have power to kick themselves apart and resume the gaseous state. Something, then, must be done to insulate the liquefied gas, else it will retain the liquid state for too short a time to be much experimented with. How might such insulation be accomplished?

The most successful attack upon this important problem has been made by Professor Dewar. He invented a receptacle for holding liquefied gases which, while not fulfilling the ideal conditions referred to above, yet accomplishes a very remarkable degree of heat insulation. In consists of a glass vessel with double walls, the space between which is rendered a vacuum of the highest practicable degree. This vacuum, containing practically no particles of matter, cannot, of course, convey heat-impulses to or from the matter in the receptacle with any degree of rapidity. Thus one of the two possible means of heat transfer is shut off and a degree of insulation afforded the liquefied substance. But of course the other channel, ether radiation, remains. Even this may be blocked to a large extent, however, by leaving a trace of mercury vapor in the vacuum space, which will be deposited as a fine mirror on the inner surface of the chamber. This mirror serves as an admirable reflector of the heat-rays that traverse the vacuum, sending more than half of them back again. So, by the combined action of vacuum and mirror, the amount of heat that can penetrate to the interior of the receptacle is reduced to about one-thirtieth of what would enter an ordinary vessel. In other words, a quantity of liquefied gas which would evaporate in one minute from an ordinary

vessel will last half an hour in one of Professor Dewar's best vacuum vessels. Thus in one of these vessels a quantity of liquefied air, for example, can be kept for a considerable time in an atmosphere at ordinary temperature, and will only volatilize at the surface, like water under the same conditions, though of course more rapidly; whereas the same liquid in an ordinary vessel would boil briskly away, like water over a fire. Only, be it remembered, the air in "boiling" is at a temperature of about one hundred and eighty degrees below zero, so that it would instantly freeze almost any substance placed into it. A portion of alcohol poured on its surface will be changed quickly into a globule of ice, which will rattle about the sides of the vessel like a marble. That is not what one ordinarily thinks of as a "boiling" temperature.

If the vacuum vessel containing a liquefied gas be kept in a cold medium, and particularly if two vacuum tubes be placed together, so that no exposed surface of liquid remains, a portion of liquefied air, for example, may be kept almost indefinitely. Thus it becomes possible to utilize the liquefied gas for experimental investigation of the properties of matter at low temperatures that otherwise would be quite impracticable. Great numbers of such experiments have been performed in the past decade or so by all the workers with low temperatures already mentioned, and by various others, including, fittingly enough, the holder of the Rumford professorship of experimental physics at Harvard, Professor Trowbridge. The work of Professor Dewar has perhaps been the most comprehensive and varied, but the researches of Pictet, Wroblewski,

and Olzewski have also been important, and it is not always possible to apportion credit for the various discoveries accurately, since the authorities themselves are in unfortunate disagreement in several questions of priority. But in any event, such questions of exact priority have no great interest for any one but the persons directly involved. We may quite disregard them here, confining attention to the results themselves, which are full of interest.

The questions investigated have to do with the physical properties, such as electrical conductivity, magnetic condition, light-absorption, cohesion, and chemical affinities of matter at excessively low temperatures. It is found that in all these regards most substances are profoundly modified when excessively cooled. Thus if a piece of any pure metal is placed in an electric circuit and plunged into liquid air, its resistance to the passage of the electricity steadily decreases as the metal cools, until at the temperature of the liquid it is very trifling indeed. The conclusion seems to be justified that if the metal could be still further cooled until it reached the theoretical "absolute zero," or absolutely heatless condition, the electrical resistance would also be nil. So it appears that the heat vibrations of the molecules of a pure metal interfere with the electrical current. The thought suggests itself that this may be because the ether waves set up by the vibrating molecules conflict with the ether strain which is regarded by some theorists as constituting the electrical "current." But this simple explanation falters before further experiments which show, paradoxically enough, that the electrical resistance of carbon exactly reverses

what has just been said of pure metals, becoming greater and greater as the carbon is cooled. If an hypothesis were invented to cover this case there would still remain a puzzle in the fact that alloys of metals do not act at all like the pure metals themselves, the electrical resistance of such alloys being, for the most part, unaffected by changed temperature. On the whole, then, the facts of electrical conduction at low temperatures are quite beyond the reach of present explanation. They must await a fuller knowledge of molecular conditions in general than is at present available—a knowledge to which the low-temperature work itself seems one of the surest channels.

Even further beyond the reach of present explanation are the facts as to magnetic conditions at low temperatures. Even as to the facts themselves different experimenters have differed somewhat, but the final conclusion of Professor Dewar is that, after a period of fluctuation, the power of a magnet repeatedly subjected to a liquid-air bath becomes permanently increased. Various substances not markedly magnetic at ordinary temperatures become so when cooled. Among these, as Professor Dewar discovered, is liquid oxygen itself. Thus if a portion of liquid air be further cooled until it assumes a semi-solid condition, the oxygen may be drawn from the mass by a magnet, leaving a pure nitrogen jelly. These facts are curious enough, and full of suggestion, but like all other questions having to do with magnetism, they hold for the present generation the double fascination of insoluble mystery. To be sure, one may readily enough suggest that if magnetism be really a whirl in the ether, this whirl is

apparently interfered with by the waves of radiant heat; or, again, that magnetism is presumably due to molecular motions which are apparently interfered with by another kind of molecular motions which we call heat vibrations; but there is a vagueness about the terms of such guesses that leaves them clearly within the category of explanations that do not explain.

When it comes to the phenomena of light, we can, as is fitting, see our way a little more clearly, since, thanks to Thomas Young and his successors, we know pretty definitely what light really is. So when we learn that many substances change their color utterly at low temperatures—red things becoming yellow and yellow things white, for example—we can step easily and surely to at least a partial explanation. We know that the color of any object depends simply upon the particular ether waves of the spectrum which that particular substance absorbs; and it does not seem anomalous that molecules packed close together at $-180°$ of temperature should treat the ether waves differently than when relatively wide apart at an ordinary temperature. Yet, after all, that may not be the clew to the explanation. The packing of the molecules may have nothing to do with it. The real explanation may lie in the change of the ether waves sent out by the vibrating molecule; indeed, the fact that the waves of radiant heat and those of light differ only in amplitude lends color to this latter supposition. So the explanation of the changed color of the cooled substance is at best a dubious one.

Another interesting light phenomenon is found in the observed fact that very many substances become

markedly phosphorescent at low temperatures. Thus, according to Professor Dewar, "gelatine, celluloid, paraffine, ivory, horn, and india-rubber become distinctly luminous, with a bluish or greenish phosphorescence, after cooling to $-180°$ and being stimulated by the electric light." The same thing is true, in varying degrees, of alcohol, nitric acid, glycerine, and of paper, leather, linen, tortoise-shell, and sponge. Pure water is but slightly luminous, whereas impure water glows brightly. On the other hand, alcohol loses its phosphorescence when a trace of iodine is added to it. In general, colored things are but little phosphorescent. Thus the white of egg is very brilliant but the yolk much less so. Milk is much brighter than water, and such objects as a white flower, a feather, and egg-shell glow brilliantly. The most remarkable substances of all, says Professor Dewar, whom I am all along quoting, are "the platinocyanides among inorganic compounds and the ketonic compounds among organic. Ammonium platinocyanide, cooled while stimulated by arc light, glows fully at $-180°$; but on warming it glows like a lamp. It seems clear," Professor Dewar adds, "that the substance at this low temperature must have acquired increased power of absorption, and it may be that at the same time the factor of molecular friction or damping may have diminished." The cautious terms in which this partial explanation is couched suggest how far we still are from a full understanding of the interesting phenomena of phosphorescence. That a molecule should be able to vibrate in such a way as to produce the short waves of light, dissevered from the usual linking with the vibrations represented by

high temperature, is one of the standing puzzles of physics. And the demonstrated increase of this capacity at very low temperatures only adds to the mystery.

There are at least two of the low-temperature phenomena, however, that seem a little less puzzling—the facts, namely, that cohesion and rigidity of structure are increased when a substance is cooled and that chemical activity is very greatly reduced, in fact almost abolished. This is quite what one would expect *a priori*—though no wise man would dwell on his expectation in advance of the experiments—since the whole question of liquids and solids *versus* gases appears to be simply a contest between cohesive forces that are tending to draw the molecules together and the heat vibration which is tending to throw them apart. As a substance changes from gas to liquid, and from liquid to solid, contracting meantime, simply through the lessening of the heat vibrations of its molecules, we might naturally expect that the solid would become more and more tenacious in structure as its molecules came closer and closer together, and at the same time became less and less active, as happens when the solid is further cooled. And for once experiment justifies the expectation. Professor Dewar found that the breaking stress of an iron wire is more than doubled when the wire is cooled to the temperature of liquid air, and all other metals are largely strengthened, though none other to quite the same degree. He found that a spiral spring of fusible metal, which at ordinary temperature was quickly drawn out into a straight wire by a weight of one ounce, would,

when cooled to −182°, support a weight of two pounds, and would vibrate like a steel spring so long as it was cool. A bell of fusible metal has a distinct metallic ring at this low temperature; and balls of iron, tin, lead, or ivory cooled to −182° and dropped from a height, "in all cases have the rebound greatly increased. The flattened surface of the lead is only one-third what it would be at ordinary temperature." "These conditions are due solely to the cooling, and persist only while the low temperature lasts."

If this increased strength and hardness of a contracted metal are what one would expect on molecular principles, the decreased chemical activity at low temperatures is no less natural-seeming, when one reflects how generally chemical phenomena are facilitated by the application of heat. In point of fact, it has been found that at the temperature of liquid hydrogen practically all chemical activity is abolished, the unruly fluorine making the only exception. The explanation hinges on the fact that every atom, of any kind, has power to unite with only a limited number of other atoms. When the "affinities" of an atom are satisfied, no more atoms can enter into the union unless some atoms already there be displaced. Such displacement takes place constantly, under ordinary conditions of temperature, because the vibrating atoms tend to throw themselves apart, and other atoms may spring in to take the places just vacated—such interchange, in fact, constituting the essence of chemical activity. But when the temperature is reduced the heat-vibration becomes insufficient to throw the atoms apart, hence any unions they chance to have made are

permanent, so long as the low temperature is maintained. Thus it is that substances which attack one another eagerly at ordinary temperatures will lie side by side, utterly inert, at the temperature of liquid air.

Under certain conditions, however, most interesting chemical experiments have been made in which the liquefied gases, particularly oxygen, are utilized. Thus Olzewski found that a bit of wood lighted and thrust into liquid oxygen burns as it would in gaseous oxygen, and a red-hot iron wire thrust into the liquid burns and spreads sparks of iron. But more novel still was Dewar's experiment of inserting a small jet of ignited hydrogen into the vessel of liquid oxygen; for the jet continued to burn, forming water, of course, which was carried away as snow. The idea of a gas-jet burning within a liquid, and having snow for smoke, is not the least anomalous of the many strange conceptions that the low-temperature work has made familiar.

PRACTICAL RESULTS AND ANTICIPATIONS

Such are some of the strictly scientific results of the low-temperature work. But there are other results of a more directly practical kind—neither more important nor more interesting on that account, to be sure, but more directly appealing to the generality of the non-scientific public. Of these applications, the most patent and the first to be made available was the one forecast by Davy from the very first—namely, the use of liquefied gases in the refrigeration of foods. Long before the more resistant gases had been liquefied, the more manageable ones, such as ammonia and sul-

phurous acid, had been utilized on a commercial scale for refrigerating purposes. To-day every brewery and every large cold-storage warehouse is supplied with such a refrigerator plant, the temperature being thus regulated as is not otherwise practicable. Many large halls are cooled in a similar manner, and thus made comfortable in the summer. Ships carrying perishables have the safety of their cargoes insured by a refrigerator plant. In all large cities there are ice manufactories using the same method, and of late even relatively small establishments, hotels, and apartment houses have their ice-machine. It seems probable that before long all such buildings and many private dwellings will be provided with a cooling apparatus as regularly as they are now equipped with a heating apparatus.

The exact details of the various refrigerator machines of course vary, but all of them utilize the principles that the laboratory workers first established. Indeed, the entire refrigerator industry, now assuming significant proportions, may be said to be a direct outgrowth of that technical work which Davy and Faraday inaugurated and prosecuted at the Royal Institution—a result which would have been most gratifying to the founder of the institution could he have forecast it. The usual means of distributing the cooling fluids in the commercial plants is by the familiar iron pipes, not dissimilar in appearance (when not in operation) to the familiar gas, water, and steam pipes. When operating, however, the pipes themselves are soon hidden from view by the thick coating of frost which forms over them. In a moist beer-cellar this coating

is often several inches in thickness, giving a very characteristic and unmistakable appearance.

Another commercial use to which refrigerator machines are now put is in the manufacture of various drugs, where absolute purity is desirable. As different substances congeal at different temperatures, but the same substances at uniform pressure always at the same temperature, a means is afforded of freeing a drug from impurities by freezing, where sometimes the same result cannot be accomplished with like thoroughness by any other practicable means. Indeed, by this means impurities have been detected where not previously suspected. And Professor Ramsay has detected some new elementary substances even, as constituents of the air, which had previously not been dissociated from the nitrogen with which they are usually mixed.

Such applications of the refrigerator principles as these, however, though of vast commercial importance, are held by many enthusiasts to be but a bagatelle compared with other uses to which liquefied gases may some time be put. Their expectations are based upon the enormous potentialities that are demonstrably stored in even a tiny portion of, say, liquefied air. These are, indeed, truly appalling. Consider, for example, a portion of air at a temperature above its critical point, to which, as in Thilorier's experiments, a pressure of thirty-one tons to the square inch of the encompassing wall is being applied. Recall that action and reaction are equal, and it is apparent that the gas itself is pushing back—struggling against being compressed, if you will—with an equal power. Suppose

the bulk of the gas is such that at this pressure it occupies a cubical space six inches on a side—something like the bulk of a child's toy balloon, let us say. Then the total outward pressure which that tiny bulk of gas exerts, in its desperate molecular struggle, is little less than five thousand tons. It would support an enormous building without budging a hair's-breadth. If the building weighed less than five thousand tons it would be lifted by the gas; if much less it would be thrown high into the air as the gas expanded. It gives one a new sense of the power of numbers to feel that infinitesimal atoms, merely by vibrating in unison, could accomplish such a result.

But now suppose our portion of gas, instead of being placed under our hypothetical building, is plunged into a cold medium, which will permit its heat-vibrations to exhaust themselves without being correspondingly restored. Then, presently, the temperature is lowered below the critical point, and, presto! the mad struggle ceases, the atoms lie amicably together, and the gas has become a liquid. What a transformed thing it is now. Instead of pressing out with that enormous force, it has voluntarily contracted as the five thousand tons pressure could not make it do; and it lies there now, limpid and harmless-seeming, in the receptacle, for all the world like so much water.

And, indeed, the comparison with water is more than superficial, for in a cup of water also there are wonderful potentialities, as every steam-engine attests. But an enormous difference, not in principle but in practical applications, exists in the fact that the potentialities of the water cannot be utilized until relatively

high temperatures are reached. Costly fuel must be burned and the heat applied to the water before it can avail to do its work. But suppose we were to place our portion of liquid air, limpid and water-like, in the cylinder of a locomotive, where the steam of water ordinarily enters. Then, though no fuel were burned —though the entire engine stood embedded in the snow of an arctic winter—it would be but a few moments before the liquid air would absorb even from this cold medium heat enough to bring it above its critical temperature; and, its atoms now dancing apart once more and re-exerting that enormous pressure, the piston of the engine would be driven back and then the entire cylinder burst into fragments as the gas sought exit.

In a word, then, a portion of liquid air has a store of potential energy which can be made kinetic merely by drawing upon the boundless and free supply of heat which is everywhere stored in the atmosphere we breathe and in every substance about us. The difficulty is, not to find fuel with which to vaporize it, as in case of water, but to keep the fuel from finding it whether or no. Were liquid air in sufficient quantities available, the fuel problem would cease to have any significance. But of course liquid air is not indefinitely available, and exactly here comes the difficulty with the calculations of many enthusiasts who hail liquefied gas as the motive power of the near future. For of course in liquefying the air power has been applied, for the moment wasted, and unless we can get out of the liquid more energy than we have applied to it, there is no economy of power in the transaction. Now the simplest study of the conditions, with the mechanical

theory of matter in mind, makes it clear that this is precisely what one can never hope to accomplish. Action and reaction are equal and in opposite directions at all stages of the manipulation, and hence, under the most ideal conditions, we must expect to waste as much work in condensing a gas (in actual practice more) as the condensed substance can do in expanding to the original volume. Those enthusiasts who have thought otherwise, and who have been on the point of perfecting an apparatus which will readily and cheaply produce liquid air after the first portion is produced, are really but following the old perpetual-motion-machine will-o'-the-wisp.

It does not at all follow from this, however, that the energies of liquefied air may not be utilized with enormous advantage. It is not always the cheapest form of power-transformer that is the best for all purposes, as the use of the electrical storage battery shows. And so it is quite within the possibilities that a multitude of uses may be found for the employment of liquid air as a motive power, in which its condensed form, its transportability or other properties will give it precedence over steam or electricity. It has been suggested, for example, that liquefied gas would seem to afford the motive power par excellence for the flying-machine, once that elusive vehicle is well in harness, since one of the greatest problems here is to reduce the weight of the motor apparatus. In a less degree the same problem enters into the calculations of ships, particularly ships of war; and with them also it may come to pass that a store of liquid air (or other gas) may come to take the place of a far heavier store of coal. It is

even within the possibilities that the explosive powers of the same liquid may take the place of the great magazines of powder now carried on war-ships; for, under certain conditions, the liquefied gas will expand with explosive suddenness and violence, an "explosion" being in any case only a very sudden expansion of a confined gas. The use of the compressed air in the dynamite guns, as demonstrated in the Cuban campaign, is a step in this direction. And, indeed, the use of compressed air in many commercial fields already competing with steam and electricity is a step towards the use of air still further compressed, and cooled, meantime, to a condition of liquidity. The enormous advantages of the air actually liquefied, and so for the moment quiescent, over the air merely compressed, and hence requiring a powerful retort to hold it, are patent at a glance. But, on the other hand, the difficulty of keeping it liquid is a disadvantage that is equally patent. How the balance will be struck between these contending advantages and disadvantages it remains for the practical engineering inventors of the future—the near future, probably—to demonstrate.

Meantime there is another line of application of the ideas which the low-temperature work has brought into prominence which has a peculiar interest in the present connection because of its singularly Rumfordian cast, so to speak. I mean the idea of the insulation of cooled or heated objects in the ordinary affairs of life, as, for example, in cooking. The subject was a veritable hobby with the founder of the Royal Institution all his life. He studied the heat-transmitting and heat-reflecting properties of various substances, in-

cluding such directly practical applications as rough surfaces *versus* smooth surfaces for stoves, the best color for clothing in summer and in winter, and the like. He promulgated his ideas far and wide, and demonstrated all over Europe the extreme wastefulness of current methods of using fuel. To a certain extent his ideas were adopted everywhere, yet on the whole the public proved singularly apathetic; and, especially in America, an astounding wastefulness in the use of fuel is the general custom now as it was a century ago. A French cook will prepare an entire dinner with a splinter of wood, a handful of charcoal, and a half-shovelful of coke, while the same fuel would barely suffice to kindle the fire in an American cook-stove. Even more wonderful is the German stove, with its great bulk of brick and mortar and its glazed tile surface, in which, by keeping the heat in the room instead of sending it up the chimney, a few bits of compressed coal do the work of a hodful.

It is one merit of the low-temperature work, I repeat, to have called attention to the possibilities of heat insulation in application to "the useful purposes of life." If Professor Dewar's vacuum vessel can reduce the heat-transmitting capacity of a vessel by almost ninety-seven per cent., why should not the same principle, in modified form, be applied to various household appliances—to ice-boxes, for example, and to cooking utensils, even to ovens and cook-stoves? Even in the construction of the walls of houses the principles of heat insulation might advantageously be given far more attention than is usual at present; and no doubt will be so soon as the European sense of economy shall be brought home to

the people of the land of progress and inventions. The principles to be applied are already clearly to hand, thanks largely to the technical workers with low temperatures. It remains now for the practical inventors to make the "application to the useful purposes of life." The technical scientists, ignoring the example which Rumford and a few others have set, have usually no concern with such uninteresting concerns.

For the technical scientists themselves, however, the low-temperature field is still full of inviting possibilities of a strictly technical kind. The last gas has indeed been liquefied, but that by no means implies the last stage of discovery. With the successive conquest of this gas and of that, lower and lower levels of temperature have been reached, but the final goal still lies well beyond. This is the north pole of the physicist's world, the absolute zero of temperature—the point at which the heat-vibrations of matter are supposed to be absolutely stilled. Theoretically this point lies 272° below the Centigrade zero. With the liquefaction of hydrogen, a temperature of about −253° or −254° Centigrade has been reached. So the gap seems not so very great. But like the gap that separated Nansen from the geographical pole, it is a very hard road to travel. How to compass it will be the study of all the low-temperature explorers in the immediate future. Who will first reach it, and when, and how, are questions for the future to decide.

And when the goal is reached, what will be revealed? That is a question as full of fascination for the physicist as the north-pole mystery has ever been for the gen-

erality of mankind. In the one case as in the other, any attempt to answer it to-day must partake largely of the nature of a guess, yet certain forecasts may be made with reasonable probability. Thus it can hardly be doubted that at the absolute zero all matter will have the form which we term solid; and, moreover, a degree of solidity, of tenacity and compactness greater than ever otherwise attained. All chemical activity will presumably have ceased, and any existing compound will retain unaltered its chemical composition so long as absolute zero pertains; though in many, if not in all cases, the tangible properties of the substance—its color, for example, and perhaps its crystalline texture—will be so altered as to be no longer recognizable by ordinary standards, any more than one would ordinarily recognize a mass of snowlike crystals as air.

It has, indeed, been suggested that at absolute zero all matter may take the form of an impalpable powder, the forces of cohesion being destroyed with the vibrations of heat. But experiment seems to give no warrant to this forecast, since cohesion seems to increase exactly in proportion to the decrease of the heat-vibrations. The solidity of the meteorites which come to the earth out of the depths of space, where something approaching the zero temperature is supposed to prevail, also contradicts this assumption. Still less warrant is there for a visionary forecast at one time entertained that at absolute zero matter will utterly disappear. This idea was suggested by the observation, which first gave a clew to the existence of the absolute zero, that a gas at ordinary temperatures and

at uniform pressure contracts by 1-272d of its own bulk with each successive degree of lowered temperature. If this law held true for all temperatures, the gas would apparently contract to nothingness when the last degree of temperature was reached, or at least to a bulk so insignificant that it would be inappreciable by standards of sense. But it was soon found by the low-temperature experimenters that the law does not hold exactly at extreme temperatures, nor does it apply at all to the rate of contraction which the substance shows after it assumes the liquid and solid conditions. So the conception of the disappearance of matter at zero falls quite to the ground.

But one cannot answer with so much confidence the suggestion that at zero matter may take on properties hitherto quite unknown, and making it, perhaps, differ as much from the conventional solid as the solid differs from the liquid, or this from the gas. The form of vibration which produces the phenomena of temperature has, clearly, a determining share in the disposal of molecular relations which records itself to our senses as a condition of gaseousness, liquidity, or solidity; hence it would be rash to predict just what inter-molecular relations may not become possible when the heat-vibration is altogether in abeyance. That certain other forms of activity may be able to assert themselves in unwonted measure seems clearly forecast in the phenomena of increased magnetism, and of phosphorescence at low temperatures above outlined. Whether still more novel phenomena may put in an appearance at the absolute zero, and if so, what may be their nature, are questions that must await the verdict of ex-

periment. But the possibility that this may occur, together with the utter novelty of the entire subject, gives the low-temperature work precedence over almost every other subject now before the world for investigation (possible exceptions being radio-activity and bacteriology). The quest of the geographical pole is but a child's pursuit compared with the quest of the absolute zero. In vital interest the one falls as far short of the other as the cold of frozen water falls short of the cold of frozen air.

Where, when, and by whom the absolute zero will be first reached are questions that may be answered from the most unexpected quarter. But it is interesting to know that great preparations are being made today in the laboratories of the Royal Institution for a further attack upon the problem. Already the research equipment there is the best in the world in this field, and recently this has been completely overhauled and still further perfected. It would not be strange, then, in view of past triumphs, if the final goal of the low-temperature workers should be first reached in the same laboratory where the outer territories of the unknown land were first penetrated three-quarters of a century ago. There would seem to be a poetic fitness in the trend of events should it so transpire. But of course poetic fitness does not always rule in the land of science.

IV

SOME PHYSICAL LABORATORIES AND PHYSICAL PROBLEMS

SIR NORMAN LOCKYER AND SOLAR CHEMISTRY

SIR NORMAN LOCKYER is professor of astronomical physics and director of the solar observatory at the Royal College of Science in South Kensington. Here it is that his chief work has been done for some thirty years past. The foundation-stone of that work is spectroscopic study of the sun and stars. In this study Professor Lockyer was a pioneer, and he has for years been recognized as the leader. But he is no mere observer; he is a generalizer as well; and he long since evolved revolutionary ideas as to the origin of the sidereal and solar systems.

For a man whose chief occupation is the study of the sun and stars, smoky, foggy, cloudy London may seem a strange location. I asked Professor Lockyer about this, and his reply was most characteristic. "The fact is," he said, "the weather here is too fine from one point of view: my working staff is so small, and the number of working nights so large, that most of the time there is no one about to do anything during the day. Then, another thing, here at South Kensington I am in touch with my colleagues in the other departments—physics, chemistry, and so forth—and

can at once draw upon their special knowledge for aid on any obscure point in their lines that may crop up. If we were out in the country this would not be so. You see, then, that it is a choice between weather and brains. I prefer the brains."

Professor Lockyer went on to state, however, that he is by no means altogether dependent upon the observations made at South Kensington. For certain purposes the Royal Observatory at Greenwich is in requisition, and there are three observatories at different places in India at which photographs of the sun-spots and solar spectra are taken regularly. From these combined sources photographs of the sun are forthcoming practically every day of the year; to be accurate, on three hundred and sixty days out of the three hundred and sixty-five. It was far otherwise when Professor Lockyer first began his studies of the sun, as observations were then made and recorded on only about one-third of the days in each year.

Exteriorly the observatory at South Kensington is not at all such a place as one might expect to find. It is, in Professor Lockyer's own words, "little more than a collection of sheds," but within these alleged sheds may be found an excellent equipment of telescopes, both refracting and reflecting, and of all other things requisite to the peculiar study which forms the subject of special research here.

I have had occasion again and again to call attention to this relatively meagre equipment of the European institutions, but in no case, perhaps, is the contrast more striking between the exterior appearance of a famous scientific institution and the work that is

SIR NORMAN LOCKYER

being accomplished within it than is shown in the case of the South Kensington observatory. It should be added that this remark does not apply to the chief building of the Royal College of Science itself.

The theories for which Professor Lockyer has so long been famous are well known to every one who takes much interest in the progress of scientific ideas. They are notably the theory that there is a direct causal association between the prevalence of sun-spots and terrestrial weather; the theory of the meteoritic origin of all members of the sidereal family; and the dissociation theory of the elements, according to which our so-called elements are really compounds, capable of being dissociated into simpler forms when subjected to extreme temperatures, such as pertain in many stars. As I have said, these theories are by no means new. Professor Lockyer has made them familiar by expounding them for a full quarter of a century or more. But if not new, these theories are much too important to have been accepted at once without a protest from the scientific world. In point of fact, each of them has been met with most ardent opposition, and it would, perhaps, not be too much to say that not one of them is, as yet, fully established. It is of the highest interest to note, however, that the multitudinous observations bearing upon each of these topics during the past decade have tended, in Professor Lockyer's opinion, strongly to corroborate each one of these opinions.

Two or three years ago Sir Norman Lockyer, in association with his son, communicated to the Royal Society a paper in which the data recently obtained as

to the relation between sun-spots and the weather in India—the field of observations having been confined to that territory—are fully elaborated. A remarkable feature of the recent work in that connection has been the proof, or seeming proof, that the temperature of the sun fluctuates from year to year. At times when the sun-spots are numerous and vigorous in their action, the spectrum of the elements in these spots becomes changed. During the times of minimum sun-spot activity the spectrum shows, for example, the presence of large quantities of iron in these spots—of course in a state of vapor. But in times of activity this iron disappears, and the lines which previously vouched for it are replaced by other lines spoken of as the enhanced lines of iron—that is to say, the lines which are believed to represent the unknown substance or substances into which the iron has been decomposed; and what is true of iron is true of various other elements that are detected in the sun-spots. The explanation of this phenomena, if Professor Lockyer reads the signs aright, is that during times of minimum sun-spot activity the temperature of the sun-spots is relatively cool, and that in times of activity the temperature becomes greatly increased. One must come, therefore, to speaking of hot spots and cool spots on the sun; although the cool spots, it will be understood, would hardly be considered cool in the terrestrial sense, since their temperature is sufficient to vaporize iron.

Now the point of the recent observations is that the fluctuations in the sun's heat, due to the periodic increase and subsidence of sun-spot disturbances—such fluctuations having been long recognized as having

regular cyclic intervals of about eleven years—are instrumental in effecting changes in the terrestrial weather. According to the paper just mentioned, it would appear to be demonstrated that the periods of decreased rainfall in India have a direct and relatively unvarying relationship to the prevalence of the sun-spots, and that, therefore, it has now become possible, within reasonable limits, to predict some years in advance the times of famine in India. So important a conclusion as this is certainly not to be passed over lightly, and all the world, scientific and unscientific alike, will certainly watch with acute interest for the verification of this seemingly startling practical result of so occult a science as solar spectroscopy.

The theory of the decomposition of the elements is closely bound up with the meteoritic theory. In a word, it may be said of each that Professor Lockyer is firmly convinced that all the evidence that has accumulated in recent years is so strongly in favor as to bring these theories almost to a demonstration. The essence of the meteoritic theory, it will be recalled, is that all stars have their origin in nebulæ which consist essentially of clouds of relatively small meteorites. It will be recalled further that Professor Lockyer long ago pointed out that stars pass through a regular series of changes as to temperature, with corresponding changes of structure, becoming for a time hotter and hotter until a maximum is reached, and then passing through gradual stages of cooling until their light dies out altogether. Very recently Professor Lockyer has been enabled, through utilization of the multiform records accumulated during years of study, to define

the various typical stages of the sidereal evolution; and not merely to define them but to illustrate them practically by citing stars which belong to each of these stages, and to give them yet clearer definition by naming the various elements which the spectroscope reveals as present in each.

His studies have shown that the elements do not always give the same spectrum under all conditions; a result quite at variance with the earlier ideas on the subject. Even in the terrestrial laboratory it is possible to subject various metals, including iron, to temperatures attained with the electric spark at which the spectrum becomes different from that, for example, which was attained with the lower temperature of the electric arc. Through these studies so-called series-spectra have been attained for various elements, and a comparison of these series-spectra with the spectra of various stars has led to the conclusion that many of the unknown lines previously traced in the spectra of such stars are due to the decomposition products of familiar elements; all of which, of course, is directly in line of proof of the dissociation hypothesis.

Another important result of Professor Lockyer's very recent studies has come about through observation of the sun in eclipse. A very interesting point at issue all along has been the question as to what layers of the sun's atmosphere are efficient in producing the so-called reverse lines of the spectrum. It is now shown that the effect is not produced, as formerly supposed, by the layers of the atmosphere lying just above the region which Professor Lockyer long ago named the chromosphere, but by the gases of higher

regions. Reasoning from analogy, it may be supposed that a corresponding layer of the atmosphere of other stars is the one which gives us the reverse spectrum of those stars. The exact composition of this layer of the sidereal atmosphere must, of course, vary with the temperature of the different stars, but in no case can we expect to receive from the spectroscope a full record of all the substances that may be present in other layers of the atmosphere or in the body of the star itself. Thus, for example, the ordinary Freuenhofer spectrum of the sun shows us no trace of the element helium, though through other observations at the time of eclipse Professor Lockyer had discovered that element there, as we have seen, some thirty years before anything was known of it on the earth.

In a recent eclipse photographs were taken of the spectra of the lower part of the sun's atmosphere by itself, and it was found that the spectrum of this restricted area taken by itself gave the lines which specialize the spectra of so different a star as Procyon. "I recognize in the result," says Professor Lockyer, "a veritable Rosetta Stone which will enable us to read the celestial hieroglyphics presented to us in stellar spectra, and help us to study the spectra and to get at results much more distinctly and certainly than ever before."

But the most striking confirmation which the meteoritic hypothesis has received has come to hand through study of the spectrum of the new star which appeared in the constellation Perseus in February, 1901, and which was so widely heralded everywhere in the public press. This star was discovered on the

morning of February 22d by star-gazers in Scotland, and in America almost simultaneously. It had certainly not been visible a few hours before, and it had blazed up suddenly to a greater brilliancy than that of a first-magnitude star. At first it was bluish-white in color, indicating an extremely high temperature, but it rapidly subsided in brilliancy and assum d a red color as it cooled, passing thus, in the course of a few days, through stages for which ordinary stars require periods of many millions of years.

The most interesting feature of the spectrum of this new star was the fact that it showed both light and dark lines for the same substances, the two lying somewhat apart. This means, being interpreted, that some portions of a given substance are giving out light, thus producing the bright lines of the spectrum, and that other portions of the same substance are stopping certain rays of transmitted light, thus producing the dark lines. The space between the bright and dark lines, being measured, indicated that there was a differential motion between the two portions of substance thus recorded of something like seven hundred miles a second. This means, according to theory —and it seems hardly possible to explain it otherwise— that two sidereal masses, one at least of which was moving at an enormous rate of speed, had collided, such collision, of course, being the cause of the incandescence that made the mass suddenly visible from the earth as a new star.

New stars are by no means every-day affairs, there having been but thirty-two of them recorded in the world's history, and of these only two have exceeded

the present one in brilliancy. As a mere spectacle, therefore, this new star was of great interest; but a far greater importance attaches to it through the fact that it conforms so admirably to the course that meteoritic hypothesis would predict for it. "That is what confounds my opponents," said Professor Lockyer, in talking to me about the new star. "Most of those who oppose my theory have not taken the trouble to make observations for themselves, but have contented themselves with falling back apparently on the postulate that because a theory is new it must be wrong. Then, outside the scientific world, comparatively few people appreciate the extreme parsimony of nature. They expect, therefore, that when such a phenomenon as the appearance of a new star occurs, the new-comer will establish new rules for itself and bring chaos into the scientific world. But in point of fact nature never does things in two ways if she can possibly do them in one, and the most striking thing about the new stars is that all the phenomena they present conform so admirably to the laws built up through observation of the old familiar stars. As to our particular theories, we here at South Kensington"—it will be understood that this use of the editorial "we" is merely a modest subterfuge on the part of Professor Lockyer—"have no regard for them at all simply as ours. Like all scientists worthy the name, we seek only the truth, and should new facts come along that seem to antagonize our theory we should welcome them as eagerly as we welcome all new facts of whatever bearing. But the truth is that no such new facts have appeared in all these years, but that, on the contrary, the me-

teoritic hypothesis has received ever-increasing support from most unexpected sources, from none more brilliantly or more convincingly than from this new star in Perseus." And I suspect that as much as this at least—if not indeed a good deal more—will be freely admitted by every candid investigator of Sir Norman Lockyer's theory.

SIR WILLIAM RAMSAY AND THE NEW GASES

The seat of Sir William Ramsay's labors is the University College, London. The college building itself, which is located on Gower Street, is, like the British Museum, reminiscent or rather frankly duplicatory in its columned architecture of the classical. Interiorly it is like so many other European institutions in its relative simplicity of equipment. One finds, for example, Professor Ramsay and Dr. Travers generating the hydrogen for their wonderful experiments in an old beer-cask. Professor Ramsay himself is a tall, rather spare man, just entering the gray stage of life, with the earnest visage of the scholar, the keen, piercing eye of the investigator—yet not without a twinkle that justifies the lineage of the "canny Scot." He is approachable, affable, genial, full of enthusiasm for his work, yet not taking it with such undue seriousness as to rob him of human interest—in a word, the type of a man of science as one would picture him in imagination, and would hope, with confident expectation, to find him in reality.

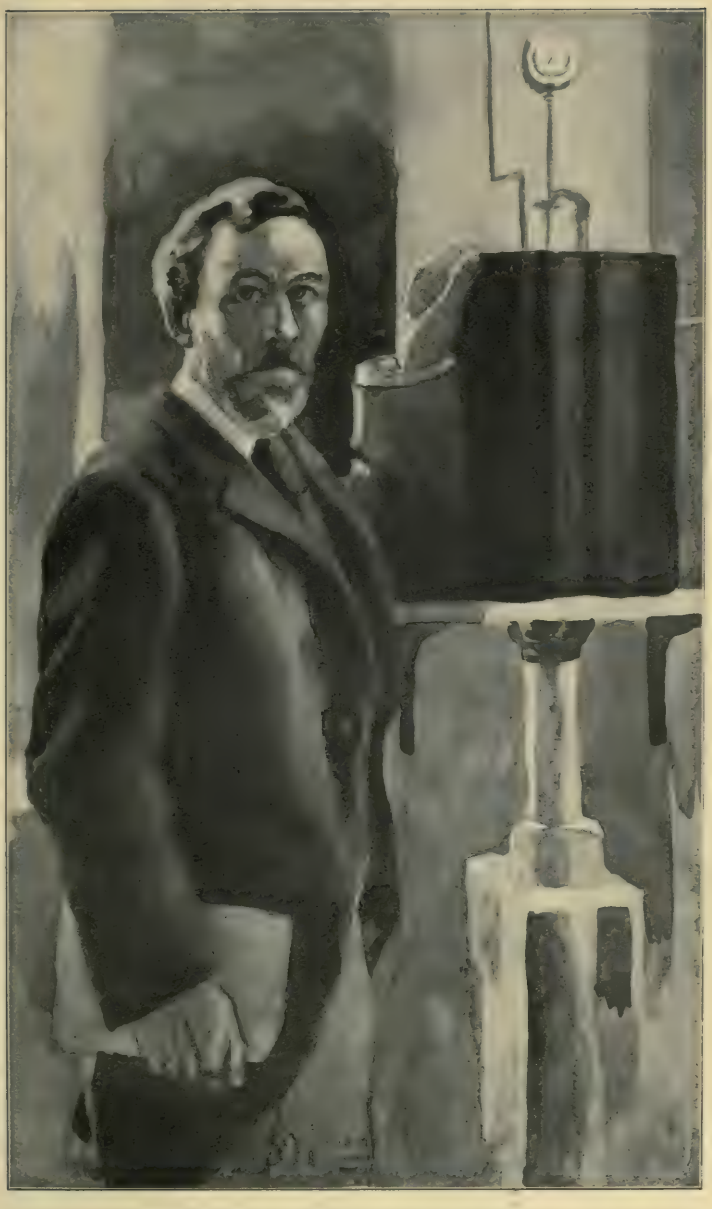
I have said that the equipment of the college is somewhat primitive, but this must not be taken too comprehensively. Such instances as that of the beer-

cask show, to be sure, an adaptation of means to ends on economical lines; yet, on the other hand, it should not be forgotten that the beer-cask serves its purpose admirably; and, in a word, it may be said that Professor Ramsay's laboratory contains everything that is needed to equip it fully for the special work to which it has been dedicated for some years past. In general, it looks like any other laboratory—glass tubes, Bunsen burners, retorts and jars being in more or less meaningless tangles; but there are two or three bits of apparatus pretty sure to attract the eye of the casual visitor which deserve special mention. One of these is a long, wooden, troughlike box which extends across the room near the ceiling and is accessible by means of steps and a platform at one end. Through this box-like tube the chief expert in spectroscopy (Dr. Bayley) spies on the spectrum of the gas, and learns some of its innermost secrets. But an even more mystifying apparatus is an elaborate array of long glass tubes, some of them carried to the height of several feet, interspersed with cups of mercury and with thermometers of various sizes and shapes. The technical scientist would not make much of this description, but neither would an untechnical observer make much of the apparatus; yet to Dr. Travers, its inventor, it is capable of revealing such extraordinary things as the temperature of liquid hydrogen—a temperature far below that at which the contents of even an alcoholic thermometer are solidified; at which, indeed, the prime constituents of the air suffer a like fate. The responsible substance which plays the part of the familiar mercury, or alcohol, in Dr. Travers's marvellous ther-

mometer is hydrogen gas. The principle by which it is utilized does not differ, in its rough essentials, from that of ordinary thermometers, but the details of its construction are much too intricate to be elaborated here.

But if you would see the most wonderful things in this laboratory—or rather, to be quite accurate, I should say, if you would stand in the presence of the most wonderful things—you must go with Professor Ramsay to his own private laboratory, and be introduced to some little test-tubes that stand inverted in cups of mercury decorating a shelf at one end. You would never notice these tubes of your own accord were you to browse ever so long about the room. Even when your attention is called to them you still see nothing remarkable. These are ordinary test-tubes inverted over ordinary mercury. They contain something, since the mercury does not rise in them completely, but if that something be other than ordinary air there is nothing about its appearance, or rather lack of appearance, to demonstrate it. But your interest will hardly fail to be arrested when Professor Ramsay, indicating one and another of these little tubes, says: "Here you see, or fail to see, all the krypton that has ever been in isolated existence in the world, and here all the neon, and here, again, all the zenon."

You will understand, of course, that krypton, neon, and zenon are the new gases of the atmosphere whose existence no one suspected until Professor Ramsay ferreted them out a few years ago and isolated them. In one sense there should be nothing mysterious about substances that every air-breathing creature on the



SKETCH OF SIR WILLIAM RAMSAY

globe has been imbibing pretty constantly ever since lungs came into fashion. But in another view the universal presence of these gases in the air makes it seem all the more wonderful that they could so long have evaded detection, considering that chemistry has been a precise science for more than a century. During that time thousands of chemists have made millions of experiments in the very midst of these atmospheric gases, yet not one of the experimenters, until recently, suspected their existence. This proves that these gases are no ordinary substances—common though they be. Personally I have examined many scientific exhibits in many lands, but nowhere have I seen anything that filled my imagination with so many scientific visions as these little harmless test-tubes at the back of Professor Ramsay's desk. Perhaps I shall attempt to visualize some of these imaginings before finishing this paper, but for the moment I wish to speak of the *modus operandi* of the discovery of these additions to the list of elements.

The discovery of argon came about in a rather singular way. Lord Rayleigh, of the Royal Institution, had noticed in experiments with nitrogen that when samples of this element were obtained from chemicals, such samples were uniformly about one per cent. lighter in weight than similar quantities of nitrogen obtained from the atmosphere. This discrepancy led him to believe that the atmospheric nitrogen must contain some impurity.

Curiously enough, the experiments of Cavendish, the discoverer of nitrogen — experiments made more than a century ago—had seemed to show quite con-

clusively that some gaseous substance different from nitrogen was to be found mixed with the samples of this gas as he obtained it from the atmosphere. This conclusion of Cavendish, put forward indeed but tentatively, had been quite ignored by his successors. Now, however, it transpired, by experiments made jointly by Lord Rayleigh and Professor Ramsay, that the conclusion was quite justified, it being shown presently that there actually exists in every portion of nitrogen, as extracted from the atmosphere, a certain quantity of another gas, hitherto unknown, and which now received the name of argon. It will be recalled with what astonishment the scientific and the unscientific world alike received the announcement made to the Royal Society in 1895 of the discovery of argon, and the proof that this hitherto unsuspected constituent of the atmosphere really constitutes about one per cent. of the bulk of atmospheric nitrogen, as previously estimated.

The discovery here on the earth of a substance which Professor Lockyer had detected as early as 1868 in the sun, and which he had provisionally named helium, excited almost equal interest; but this element was found in certain minerals, and not as a constituent of the atmosphere.

Having discovered so interesting a substance as argon, Professor Ramsay and his assistants naturally devoted much time and attention to elucidating the peculiarities of the new substance. In the course of these studies it became evident to them that the presence of argon alone did not fully account for all the phenomena they observed in handling liquefied air, and

in 1898 Professor Ramsay was again able to electrify his audience at the Royal Society by the announcement of the discovery, in pretty rapid succession, of three other elementary substances as constituents of the atmosphere, these three being the ones just referred to—krypton, neon, and zenon.

It is a really thrilling experience, standing in the presence of the only portions of these new substances that have been isolated, to hear Professor Ramsay and Dr. Travers, his chief assistant, tell the story of the discovery—how they worked more and more eagerly as they found themselves, so to say, on a "warmer scent," following out this clew and that until the right one at last brought the chase to a successful issue. "It was on a Sabbath morning in June, if I remember rightly, when we finally ran zenon down," says Dr. Travers, with a half smile; and Professor Ramsay, his eyes twinkling at the recollection of this very unorthodox procedure, nods assent. "And have you got them all now?" I queried, after hearing the story. "Yes; we think so," replied Professor Ramsay. "And I am rather glad of it," he adds, with a half sigh, "for it was wearisome even though fascinating work." Just how wearisome it must have been only a professional scientific investigator can fully comprehend; but the fascination of it all may be comprehended in some measure by every one who has ever attempted creative work of whatever grade or in whatever field.

I have just said that the little test-tubes contain the only bit of each of the substances named that has ever been isolated. This statement might lead the un-

technical reader to suppose that these substances, once isolated, have been carefully stored away and jealously guarded, each in its imprisoning test-tubes. Jealously guarded they have been, to be sure, but there has not been, by any means, the solitary confinement that the words might seem to imply. On the contrary, each little whiff of gas has been subjected to a variety of experiments—made to pass through torturing-tubes under varying conditions of temperature, and brought purposely in contact with various other substances, that its physical and chemical properties might be tested. But in each case the experiment ended with the return of the substance, as pure as before, to its proper tube. The precise results of all these experiments have been communicated to the Royal Society by Professor Ramsay. Most of these results are of a technical character, hardly appealing to the average reader. There is one very salient point, however, in regard to which all the new substances, including argon and helium, agree; and it is that each of them seems to be, so far as present experiments go, absolutely devoid of that fundamental chemical property, the power to combine with other elements. All of them are believed to be monatomic—that is to say, each of their molecules is composed of a single atom. This, however, is not an absolutely novel feature as compared with other terrestrial elements, for the same thing is true, for example, of such a familiar substance as mercury. But the incapacity to enter into chemical combinations seems very paradoxical; indeed it is almost like saying that these are chemical elements which lack the most fundamental of chemical properties.

It is this lack of combining power, of course, that explains the non-discovery of these elements during all these years, for the usual way of testing an element is to bring it in contact with other substances under conditions that permit its atoms to combine with other atoms to the formation of new substances. But in the case of new elements such experiments as this have not proved possible under any conditions as yet attained, and reliance must be had upon other physical tests—such as variation of the bulk of the gas under pressure, and under varying temperatures, and a study of the critical temperatures and pressures under which each gas becomes a liquid. The chief reliance, however, is the spectroscope—the instrument which revealed the presence of helium in the sun and the stars more than a quarter of a century before Professor Ramsay ferreted it out as a terrestrial element. Each whiff of colorless gas in its test-tube interferes with the light passing through it in such a way that when viewed through a prism it gives a spectrum of altogether unique lines, which stamp it as krypton, neon, or zenon as definitely as certain familiar and more tangible properties stamp the liquid which imprisons it as mercury.

Queries Suggested by the New Gases

Suppose that a few years ago you had asked some chemist, "What are the constituents of the atmosphere?" He would have responded, with entire confidence, "Oxygen and nitrogen chiefly, with a certain amount of water-vapor and of carbonic-acid gas and a trace of ammonia." If questioned as to the chief prop-

erties of these constituents, he would have replied, with equal facility, that these are among the most important elements; that oxygen might almost be said to be the life-giving principle, inasmuch as no air-breathing creature could get along without it for many moments together; and that nitrogen is equally important to the organism, though in a different way, inasmuch as it is not taken up through the lungs. As to the water-vapor, that, of course, is a compound of oxygen and hydrogen, and no one need be told of its importance, as every one knows that water makes up the chief bulk of protoplasm; carbonic-acid gas is also a compound of oxygen, the other element this time being carbon, and it plays a quite different rôle in the economy of the living organism, inasmuch as it is produced by the breaking down of tissues, and must be constantly exhaled from the lungs to prevent the poisoning of the organism by its accumulation; while ammonia, which exists only in infinitesimal quantities in the air, is a compound of nitrogen and hydrogen, introducing, therefore, no new element.

If one studies somewhat attentively the relation which these elements composing the atmosphere bear to the living organism he cannot fail to be struck with it; and it would seem a safe inductive reasoning from the stand-point of the evolutionist that the constituents of the atmosphere have come to be all-essential to the living organism, precisely because all their components are universally present. But, on the other hand, if we consider the matter in the light of these researches regarding the new gases, it becomes clear that perhaps the last word has not been said on this

subject; for here are four or five other elementary substances which, if far less abundant than oxygen and nitrogen, are no less widely distributed and universally present in the atmosphere, yet no one of which apparently takes any chemical share whatever in ministering to the needs of the living organism. This surely is an enigma.

Taking another point of view, let us try to imagine the real status of these new gases of the air. We think of argon as connected with nitrogen because in isolation experiments it remains after the oxygen has been exhausted, but in point of fact there is no such connection between argon and nitrogen in nature. The argon atom is just as closely in contact with the oxygen in the atmosphere as with the nitrogen; it simply repels each indiscriminately. But consider a little further; the argon atom not only repels all advance on the part of oxygen and nitrogen, but it equally holds itself aloof from its own particular kindred atoms. The oxygen or nitrogen atom never rests until it has sought out a fellow, but the argon atom declines all fellowship. When the chemist has played his tricks upon it, it finds itself crowded together with other atoms of the same kind; but lift up the little test-tube and these scurry off from one another in every direction, each losing its fellows forever as quickly as possible.

As one ponders this one is almost disposed to suggest that the atom of argon (or of krypton, helium, neon, or zenon, for the same thing applies to each and all of these) seems the most perfect thing known to us in the world, for it needs no companionship, it is self-

sufficing. There is something sublime about this magnificient isolation, this splendid self-reliance, this undaunted and undauntable self-sufficiency—these are traits which the world is wont to ascribe to beings more than mortal. But let us pause lest we push too far into the old, discredited territory of metaphysics.

PROFESSOR J. J. THOMPSON AND THE NATURE OF ELECTRICITY

Many fascinating questions suggest themselves in connection with these strange, new elements — new, of course, only in the sense of human knowledge— which all these centuries have been about us, yet which have managed until now to keep themselves as invisible and as intangible as spirits. Have these celibate atoms remained thus always isolated, taking no part in world-building? Are they destined throughout the sweep of time to keep up this celibate existence? And why do these elements alone refuse all fellowship, while the atoms of all the other seventy-odd known elements seek out mates under proper conditions with unvarying avidity?

It is perhaps not possible fully to answer these questions as yet, but recent studies in somewhat divergent fields give us suggestive clews to some of them. I refer in particular to the studies in reference to the passage of electricity through liquids and gases and to the observations on radio-activity. The most conspicuous worker in the field of electricity is Professor J. J. Thompson, who for many years has had charge of the Cavendish laboratory at Cambridge. In briefly reviewing certain phases of his work we shall find our-

selves brought into contact with some of the same problems raised by workers in the other fields of physics, and shall secure some very interesting bits of testimony as to the solution of questions already outlined.

The line of observation which has led to the most striking results has to do, as already suggested, with the conduction of electricity through liquids and gases. It has long been known that many liquids conduct electricity with relative facility. More recently it has been observed that a charge of electricity carried by any liquid bears a curious relation to the atomic composition of that liquid. If the atom in question is one of the sort that can combine with only a single other atom (that is to say, a monovalent atom), each atom conveys a unit charge, which is spoken of as an ion of electricity. But if a divalent atom is in question the charge carried is double, and, similarly, a trivalent atom carries a triple charge. As there are no intermediate charges it is obvious that here a very close relation is suggested between electrical units and the atomic units of matter.

This, however, is only a beginning. Far more interesting are the results obtained by the study of gases in their relation to the conduction of electricity. As is well known, gases under ordinary conditions are non-conductors. But there are various ways in which a gas may be changed so as to become a conductor; for example, by contact with incandescent metals or with flame, or by treating with ultra-violet light, with Röntgen rays, or with the rays of a radio-active substance. Now the all-important question is as to just what change has taken place in the gas so treated to

make it a conductor of electricity. I cannot go into details here as to the studies that have been addressed to the answer of this question, but I will briefly epitomize what, for our present purpose, are the important results. First and foremost of these is the fact that a gas thus rendered conductive contains particles that can be filtered out of it by passing the gas through wool or through water. These particles are the actual agents of conduction of electricity, since the gas when filtered ceases to be conductive. But there is another way in which the particles may be removed—namely, by action of electricity itself. If the gas be caused to pass between two metal plates, one of them insulated and attached to an electrometer, a charge of positive electricity at high potential sent through the other plate will drive part of the particles against the insulated plate. This proves that the particles in question are positively electrified. The amount of the charge which they carry may be measured by the electrometer.

The aggregate amount of the electrical charge carried by these minute particles in the gas being known, it is obvious that could we know the number of particles involved the simplest calculation would determine the charge of each particle. Professor Thompson devised a singularly ingenious method of determining this number. The method was based on the fact discovered by C. T. R. Wilson that charged particles acted as nuclei round which small drops of water condense much as dust particles serve the same purpose. "In dust-free air," says Professor Thompson, "as Aitken showed, it is very difficult to get a fog when damp air is cooled,

PROFESSOR J. J. THOMSON

since there are no nuclei for the drops to condense round. If there are charged particles in dust-free air, however, the fog will be deposited round these by supersaturation far less than that required to produce any appreciable fog when no charged particles are present.

"Thus, in sufficiently supersaturated damp air a cloud is deposited on these charged particles and they are thus rendered visible. This is the first step towards counting them. The drops are, however, far too small and too numerous to be counted directly. We can, however, get their number indirectly as follows: suppose we have a number of these particles in dust-free air in a closed vessel, the air being saturated with water-vapor; suppose now that we produce a sudden expansion of the air in the vessel; this will cool the air, it will be supersaturated with vapor, and drops will be deposited round the charged particles. Now if we know the amount of expansion produced we can calculate the cooling of the gas, and, therefore, the amount of water deposited. Thus we know the volume of water in the form of drops, so that if we know the volume of one drop we can deduce the number of drops. To find the size of a drop, we make use of the investigations made by Sir George Stokes on the rate at which small spheres fall through the air. In consequence of the viscosity of the air small bodies fall exceedingly slowly, and the smaller they are the slower they fall." [1]

Professor Thompson gives us the formula by which Stokes made his calculation. It is a relatively simple algebraic one, but need not be repeated here. For us it suffices that with the aid of this formula, by merely measuring the actual descent of the top of a vapor

cloud, Professor Thompson was able to find the volume of the drops and thence the number of particles. The number of particles being known, the charge of electricity carried by each could be determined, as already suggested. Experiments were made with air, hydrogen, and carbonic acid, and it was found that the particles had the same charge in all of these gases. "A strong argument," says Professor Thompson, "in favor of the atomic character of electricity." When we add that the charge in question was found to be the same as the unit charge of an ion in a liquid, it will be seen that the experiment has other points of interest and suggestiveness.

Even more interesting in some regards were the results of computation as to the actual masses of the charged particles in question. Professor Thompson found that the carrier of a negative charge could have only about one-thousandth part of the mass of a hydrogen atom, which latter had been regarded as the smallest mass able to have an independent existence. Professor Thompson gave the name corpuscle to these units of negative electricity; they are now more generally termed electrons. "These corpuscles," he says, "are the same however the electrification may have risen or wherever they may be found. Negative electricity in a gas at a low pressure has thus a structure analogous to that of a gas, the corpuscles taking the place of the molecules. The 'negative electric fluid,' to use the old notation, resembles the gaseous fluid with a corpuscular instead of a molecular structure."[2] Professor Thompson does not hesitate to declare that we now "know more about 'electric fluid'

PROFESSOR W. C. ROENTGEN
(Discoverer of the X-Ray.)

than we know about such fluids as air or water."[3] The results of his studies lead him, he declares, "to a view of electrification which has a striking resemblance to that of Franklin's *One Fluid Theory of Electricity*. Instead of taking, as Franklin did, the electric fluid to be positive electricity," he says, "we take it to be negative. The 'electric fluid' of Franklin corresponds to an assemblage of corpuscles, negative electrification being a collection of these corpuscles. The transference of electrification from one place to another is effected by the motion of corpuscles from the place where there is a gain of positive electrification to the place where there is a gain of negative. A positively electrified body is one that has lost some of its corpuscles."[4]

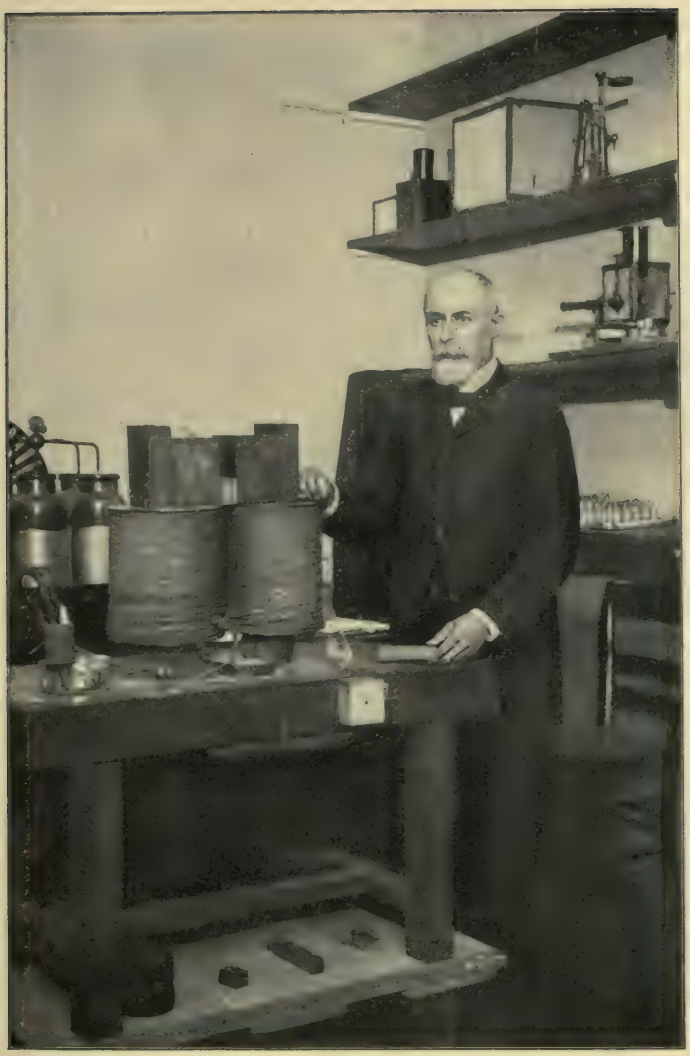
According to this view, then, electricity is not a form of energy but a form of matter; or, to be more precise, the electrical corpuscle is the fundamental structure out of which the atom of matter is built. This is a quite different view from that scarcely less recent one which regards electricity as the manifestation of ether strain, but it must be admitted that the corpuscular theory is supported by a marvellous array of experimental evidence, though it can perhaps hardly be claimed that this brings the theory to the plane of demonstration. But all roads of physical science of late years have seemed to lead towards the electron, as will be made further manifest when we consider the phenomena of radio-activity, to which we now turn.

RADIO-ACTIVITY

In 1896, something like a year after the discovery of the X-ray, Niewenglowski reported to the French

Academy of Sciences that the well-known chemical compound calcium sulphide, when exposed to sunlight, gave off rays that penetrated black paper. He had made his examinations of this substance, since, like several others, it was known to exhibit strong fluorescent or phosphorescent effects when exposed to the cathode rays, which are known to be closely connected with the X-rays. This discovery was followed very shortly by confirmatory experiments made by Becquerel, Troost, and Arnold, and these were followed in turn by the discovery of Le Bon, made almost simultaneously, that certain bodies when acted upon by sunlight give out radiations which act upon a photographic plate. These manifestations, however, are not the effect of radio-activity, but are probably the effects of short ultra-violet light waves, and are not produced spontaneously by the substances. The radiations, or emanations, of the radio-active substances, on the other hand, are given out spontaneously, pass through substances opaque to ordinary light, such as metal plates, act upon photographic plates, and discharge electrified bodies. The substances uranium, thorium, polonium, radium, and their compounds are radio-active, radium being by far the most active.

The first definite discovery of such a radio-active substance was made by M. Henri Becquerel, in 1896, while making some experiments upon the peculiar ore pitch-blende. Pitch-blende is a heavy, black, pitchy-looking mineral, found principally at present in some parts of Saxony and Bohemia on the Continent, in Cornwall in Great Britain, and in Colorado in America. It is by no means a recently discovered mineral, having



PROFESSOR HENRI BECQUEREL

been for some years the source of uranium and its compounds, which, on account of their brilliant colors, have been used in dye-stuffs and some kinds of stained glass. It is a complex mineral, containing at least eight or ten elements, which can be separated from it only with great difficulty and by complicated chemical processes.

Becquerel's discovery was brought about by a lucky accident, although, like so many other apparently accidental scientific discoveries, it was the outcome of a long series of scientific experiments all trending in the same direction. He had found that uranium, when exposed to the sun's rays, appeared to possess the property of absorbing them and of then acting upon a photographic plate. Since pitch-blende contained uranium, or uranium salts, he surmised that a somewhat similar result might be obtained with the ore itself. He therefore prepared a photographic plate wrapped in black paper, intending to attempt making an impression on the plate of some metal body interposed between it and the pitch-blende. For this purpose he had selected a key; but as the day proved to be cloudy he put the plate, with the key and pitch-blende resting upon it, in a dark drawer in his desk, and did not return to the experiment for several days. Upon doing so, however, he developed the plate without further exposure, when to his astonishment he found that the developed negative showed a distinct impression of the key. Clearly this was the manifestation of a property heretofore unknown in any natural substance, and was strikingly similar to the action of the Roentgen rays. Further investigations by Lord Kel-

vin, Beattie, Smolan, and Rutherford confirmed the fact that, like the Roentgen rays, the uranium rays not only acted upon the photographic plate but discharged electrified bodies. And what seemed the more wonderful was the fact that these "Becquerel rays," as they were now called, emanated spontaneously from the pitch-blende. But although this action is analogous to the Roentgen rays, at least as regards its action upon the photographic plate and its influence on the electric field, its action is extremely feeble in comparison, the Roentgen rays producing effects in minutes, or even seconds, which require days of exposure to uranium rays.

The discovery of the radio-active properties of uranium was followed about two years later by the discovery that thorium, and the minerals containing thorium, possess properties similar to those of uranium. This discovery was made independently and at about the same time by Schmidt and Madame Skaldowska Curie. But the importance of this discovery was soon completely overshadowed by the discovery of radium by Madame Curie, working with her husband, Professor Pierre Curie, at the École Polytechnique in Paris. Madame Curie, stimulated by her own discoveries and those of the other scientists just referred to, began a series of examinations upon various substances by numerous complicated methods to try and find a possible new element, as certain peculiarities of the substances found in the pitch-blende seemed to indicate the presence of some hitherto unknown body. The search proved a most difficult one on account of the peculiar nature of the object in question, but the tireless enthusiasm of Madame Curie knew nothing of in-

surmountable obstacles, and soon drew her husband into the search with her. Her first discovery was that of the substance polonium—so named by Madame Curie after her native country, Poland. This proved to be another of the radio-active substances, differing from any other yet discovered, but still not the sought-for element. In a short time, however, the two Curies made the great discovery of the element radium—a substance which, according to their estimate, is some one million eight hundred thousand times more radio-active than uranium. The name for this element, *radium*, was proposed by Madame Curie, who had also suggested the term "radio-activity."

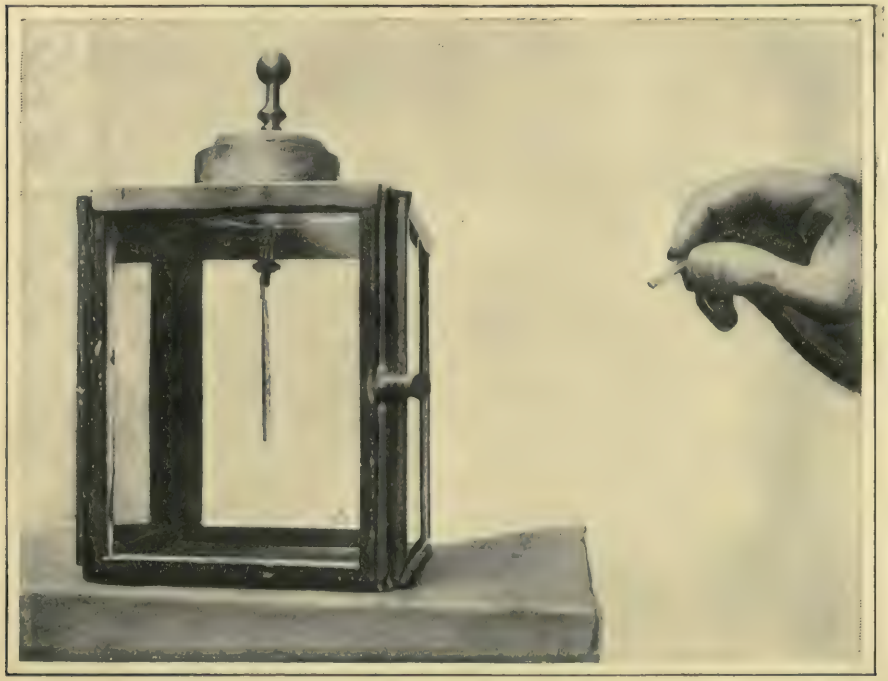
The bearing of the discovery of radium and radio-activity upon theories of the atom and matter will be considered in a moment; first the more tangible qualities of this wonderful substance may be briefly referred to. The fact that radio-active emanations traverse all forms of matter to greater or less depth—that is, pass through wood and iron with something the same ease that light passes through a window-glass—makes the subject one of greatest interest; and particularly so as the demonstration of this fact is so tangible. While the rays given out by radium cannot, of course, be seen by the unaided eye, the effects of these rays upon certain substances, which they cause to phosphoresce, are strikingly shown. One of such substances is the diamond, and a most striking illustration of the power of radium in penetrating opaque substances has been made by Mr. George F. Kunz, of the American Museum of Natural History. Mr. Kunz describes this experiment as follows:

"Radium bromide of three hundred thousand activity was placed in a sealed glass tube inside a rubber thermometer-holder, which was tightly screwed to prevent any emanation of any kind from passing through the joints. This was placed under a heavy silver tureen fully one-sixteenth of an inch in thickness; upon this were placed four copper plates, such as are used for engraving; upon these a heavy graduated measuring-glass 10 cm. in diameter; this was filled with water to a depth of six inches. A diamond was suspended in the water and immediately phosphoresced. Whenever the tube of radium was drawn away more than two or three feet the phosphoresce ce ceased; whenever it was placed under the tureen the diamond immediately phosphoresced again. This experiment proves that the active power of the radium penetrated the following substances:

"Glass in the form of a tube, sealed at both ends; the rubber thermometer-holder; silver tureen; four copper plates; a glass vase or measuring-glass one-quarter of an inch in thickness; three inches of water. There is no previously known substance or agent, whether it be even light or electricity, that possesses such wonderfully penetrative powers."[5]

The Nature of Emanations from Radio-active Bodies

What, then, is the nature of these radiations? Are they actually material particles hurled through the ether? Or are they like light — and possibly the Roentgen rays—simply undulations in the ether? As yet this question is an open one, although several of



DISCHARGING AN ELECTROSCOPE WITH RADIUM

the leading investigators have postulated tentative hypotheses which at least serve as a working basis until they are either confirmed or supplanted. On one point, however, there seems to be unanimity of opinion —there seems to be little question that there are at least three different kinds of rays produced by radio-active substances. According to Sir William Crookes, the first of these are free electrons, or matter in an ultra-gaseous state, as shown in the cathode stream. These particles are extremely minute. They carry a negative charge of electricity, and are identified with the electric corpuscles of Thompson. Rays of the second kind are comparable in size to the hydrogen atom, and are positively electrified. These are easily checked by material obstructions, although they render the air a conductor and affect photographic plates. The third are very penetrating rays, which are not deflected by electricity and which are seemingly identical with Roentgen rays. Professor E. Rutherford has named these rays beta (β), alpha (α), and gamma (γ) rays respectively. Of these the beta rays are deviated strongly by the magnetic field, the alpha much less so—very slightly, in fact—while the gamma rays are not affected at all. The action of these three different sets of rays upon certain substances is not the same, the beta and gamma rays acting strongly upon barium platinocyanide, but feebly on Sidot's blende, while the alpha rays act exactly the reverse of this, acting strongly on Sidot's blende.

If a surface is coated with Sidot's blende and held near a piece of radium nitrate, the coated surface begins to glow. If now it is examined with a lens, brill-

iant sparks or points can be seen. As the radium is brought closer and closer these sparks increase in number, until, as Sir William Crookes says, we seem to be witnessing a bombardment of flying atoms hurled from the radium against the surface of the blende. A little instrument called a spinthariscope, devised by Dr. Crookes and on sale at the instrument and optical-goods shops, may be had for a trifling sum. It is fitted with a lens focused upon a bit of Sidot's blende and radium nitrate, and in a dark room shows these beautiful scintillations "like a shower of stars." A still less expensive but similar device is now made in the form of a microscopic slide, to be used with the ordinary lens.

As we said a moment ago, radium appears to be an elementary substance, as shown by its spark-spectrum being different from that of any other known substance —the determinative test as fixed by the International Chemical Congress. A particle of radium free from impurities should, therefore, according to the conventional conception of an element, remain unchanged and unchangeable. If any such change did actually take place it would mean that the conception of the Daltonian atom as the ultimate particle of matter is definitively challenged from a new direction. This is precisely what has taken place. In July of 1903 Sir William Ramsay and Mr. Soddy, in making some experiments with radium, saw produced, apparently from radium emanations, another quite different and distinct substance, the element helium. The report of such a revolutionary phenomenon was naturally made with scientific caution. Though the observation seem-

ed to prove the actual transformation of one element into another, Professor Ramsay himself was by no means ready to declare the absolute certainty of this. Yet the presumption in favor of this interpretation of the observed phenomena is very strong; and so cautious a reasoner as Professor Rutherford has declared recently that "there can be no doubt that helium is derived from the emanations of radium in consequence of changes of some kind occurring in it."[6]

"In order to explain the presence of helium in radium on ordinary chemical lines," says Professor Rutherford, "it has been suggested that radium is not a true element, but a molecular compound of helium with some substance known or unknown. The helium compound gradually breaks down, giving rise to the helium observed. It is at once obvious that this postulated helium compound is of an entirely different character to any other compound previously observed in chemistry. Weight for weight, it emits during its change an amount of energy at least one million times greater than any molecular compound known. In addition, it must be supposed that the rate of breaking up of the helium compound is independent of great ranges of temperature—a result never before observed in any molecular change. The helium compound in its breaking up must give rise to the peculiar radiations and also pass through the successive radio-active change observed in radium. . . . On the other hand, radium, as far as it has been examined, has fulfilled every test required of an element. It has a well-marked and characteristic spectrum, and there is no

reason to suppose that it is not an element in the ordinarily accepted sense of the term."[7]

The Source of Energy of Radio-Activity

In 1903 Messrs. Curie and Laborde[8] made the remarkable announcement that a crystal of radium is persistently warmer than its surrounding medium; in other words, that it is perpetually giving out heat without apparently becoming cooler. At first blush this seemed to contradict the great physical law of the conservation of energy, but physicists were soon agreed that a less revolutionary explanation of the phenomenon is perfectly tenable. The giving off of heat is indeed only an additional evidence of the dissipation of energy to which the radio-active atom is subjected. And no one now believes that radio-activity can persist indefinitely without actually exhausting the substance of the atom. Even so, the evidence of so great a capacity to give out energy is startling, and has given rise to various theories (all as yet tentative) in explanation. Thus J. Perrin[9] has suggested that atoms may consist of parts not unlike a miniature planetary system, and in the atoms of the radio-elements the parts more distant from the centre are continually escaping from the central attraction, thus giving rise to the radiations. Monsieur and Madame Curie have suggested that the energy may be borrowed from the surrounding air in some way, the energy lost by the atom being instantly regained. Filipo Re,[10] in 1903, advanced the theory that the various parts of the atom might at first have been free particles constituting an extremely tenuous nebula.

SIR WILLIAM CROOKES

These parts gradually becoming collected around condensed centres have formed what we know as the atoms of elements, the atom thus becoming like an extinct sun of the solar system. From this point of view the radio-active atoms represent an intermediate stage between nebulæ and chemical atoms, the process of contraction giving rise to the heat emissions.

Lord Kelvin has called attention to the fact that when two pieces of paper, one white and the other black, are placed in exactly similar glass vessels of water and exposed to light, the temperature of the vessel containing the black paper is raised slightly higher than the other. This suggests the idea that in a similar manner radium may keep its temperature higher than the surrounding air by the absorption of other radiations as yet unknown.

Professor J. J. Thompson believes that the source of energy is in the atom itself and not external to it. "The reason," he says, "which induces me to think that the source of the energy is in the atom of radium itself and not external to it is that the radio-activity of substances is in all cases in which we have been able to localize it a transient property. No substance goes on being radio-active very long. It may be asked, how can this statement be reconciled with the fact that thorium and radium keep up their activity without any appreciable falling off with time. The answer to this is that, as Rutherford and Soddy have shown in the case of thorium, it is only an exceedingly small fraction of the mass which is at any one time radio-active, and that this radio-active portion loses its activity in a

few hours, and has to be replaced by a fresh supply from the non-radio-active thorium." [11]

If Professor Thompson's view be correct, the amount of potential energy inherent in the atom must be enormous.

Radio-Activity and the Structure of the Atom

But whatever the source of the energy displayed by the radio-active substances, it is pretty generally agreed that the radio-activity of the radio-elements results in the disruption of their atoms. Since all substances appear to be radio-active in a greater or less degree, it would seem that, unless there be a very general distribution of radio-active atoms throughout all substances, all atoms must be undergoing disruption. Since the distribution of radio-active matter throughout the earth is so great, however, it is as yet impossible to determine whether this may not account for the radio-activity of all substances.

As we have just seen, recent evidence seems to point to the cause of the disruption of radio-active atoms as lying in the atoms themselves. This view is quite in accord with modern ideas of the instability of certain atoms. It has been suggested that some atoms may undergo a slower disintegration without necessarily throwing off part of their systems with great velocity. It is even possible that all matter may be undergoing transformation, this transformation tending to simplify and render more stable the constituents of the earth. The radio-active bodies, however, are the only ones that have afforded an opportunity for studying this transformation. In these the rapidity of the

change would be directly proportionate to their radio-activity. Radium, according to the recent estimate of the Curies, would be disintegrating over a million times more rapidly than uranium. Since the amount of transformation occurring in radium in a year amounts to from 1-2000 to 1-10,000 of the total amount, the time required for the complete transformation of an atom of uranium would be somewhere between two billion and ten billion years—figures quite beyond the range of human comprehension.

Various hypotheses have been postulated to account for the instability of the atom. Perhaps the most thinkable of these to persons not specially trained in dealing with abstruse subjects is that of Professor Thompson. It has the additional merit, also, of coming from one of the best-known investigators in this particular field. According to this hypothesis the atom may be considered as a mass of positively and negatively charged particles, all in rapid motion, their mutual forces holding them in equilibrium. In case of a very complex structure of this kind it is possible to conceive of certain particles acquiring sufficient kinetic energy to be projected from the system. Or the constraining forces may be neutralized momentarily, so that the particle is thrown off at the same velocity that it had acquired at the instant it is released. The primary cause of this disintegration of the atom may be due to electro-magnetic radiation causing loss of energy of the atomic system.

Sir Oliver Lodge suggests that this instability of the atom may be the result of the atom's radiation of energy. "Lodge considered the simple case of a nega-

tively charged electron revolving round an atom of mass relatively large but having an equal positive charge and held in equilibrium by electrical forces. This system will radiate energy, and since the radiation of energy is equivalent to motion in a resisting medium, the particle tends to move towards the centre and its speed consequently increases. The rate of radiation of energy will increase rapidly with the speed of the electron. When the speed of the electron becomes very nearly equal to the velocity of light, according to Lodge, the system is unstable. It has been shown that the apparent mass of an electron increases very rapidly as the speed of light is approached, and is theoretically infinite at the speed of light. There will be at this stage a sudden increase of the mass of the revolving atom, and, on the supposition that this stage can be reached, a consequent disturbance of the balance of forces holding the system together. Lodge considers it probable that under these conditions the parts of the system will break asunder and escape from the sphere of one another's influence.

"It is probable," adds Rutherford, "that the primary cause of the disintegration of the atom must be looked for in the l ss of energy of the atomic system due to electro-magnetic radiation." [12]

Several methods have been devised for testing the amount of heat given off by radium and its compounds, and for determining its actual rise in temperature above that of the surrounding atmosphere. One of these methods is to place some substance, such as barium chloride, in a calorimeter, noting at what point the mercury remains stationary. Radium is then in-

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troduced, whereupon the mercury in the tube gradually rises, falling again when the radium is removed. By careful tests it has been determined that a gram of radium emits about twenty-four hundred gram-calories in twenty-four hours. On this basis a gram of radium in a year emits enough energy to dissociate about two hundred and twenty-five grams of water.

What seems most remarkable about this constant emission of heat by the radium atom is that it does not apparently draw upon external sources for it, but maintains it by the internal energy of the atom itself. This latent energy must be enormous, but is only manifested when the atom is breaking up. In this process of disruption many of the particles are thrown off; but the greater part seem to be stopped in their flight in the radium itself, so that their energy of motion is manifested in the form of heat. Thus, if this explanation is correct, the temperature of the radium is maintained above that of surrounding substances by the bombardment of its own particles. Since the earth and the atmosphere contain appreciable quantities of radio-active matter, this must play a very important part in determining the temperature of the globe—so important a part, indeed, that all former estimates as to the probable length of time during which the earth and sun will continue to radiate heat are invalidated. Such estimates, for example, as that of Lord Kelvin as to the probable heat-giving life of the sun must now be multiplied from fifty to five hundred times.

In like manner the length of time that the earth has been sufficiently cool to support animal and vegetable

life must be re-estimated. Until the discovery of radium it seemed definitely determined that the earth was gradually cooling, and would continue to cool, until, like the moon, it would become too cold to support any kind of vegetable or animal life whatever. But recent estimates of the amount of radio-active matter in the earth and atmosphere, and the amount of heat constantly given off from this source, seem to indicate that the loss of heat is (for the moment) about evenly balanced by the heat given out by radio-active matter.

Thus at the beginning of the new century we see the phenomenon of a single discovery in science completely overturning certain carefully worked out calculations, although not changing the great principles involved. It is but the repetition of the revolutionary changes that occur at intervals in the history of science, a simple discovery setting at naught some of the most careful calculations of a generation.

V

THE MARINE BIOLOGICAL LABORATORY AT NAPLES

THE AQUARIUM

MANY tourists who have gone to Naples within recent years will recall their visit to the aquarium there among their most pleasant experiences. It is, indeed, a place worth seeing. Any Neapolitan will direct you to the beautiful white building which it occupies in the public park close by the water's side. The park itself, statue-guarded and palm-studded, is one of the show-places of the city; and the aquarium building, standing isolated near its centre, is worthy of its surroundings. As seen from the bay, it gleams white amid the half-tropical foliage, with the circling rampart of hills, flanked by Vesuvius itself, for background. And near at hand the picturesque cactus growth scrambling over the walls gives precisely the necessary finish to the otherwise rather severe type of the architecture. The ensemble prepares one to be pleased with whatever the structure may have to show within.

It prepares one also, though in quite another way, for a surprise; for when one has crossed the threshold and narrow vestibule, while the gleam of the outside brightness still glows before his eyes, he is plunged

suddenly into what seems at first glimpse a cavern of Egyptian darkness, and the contrast is nothing less than startling. To add to the effect, one sees all about him, near the walls of the cavern, weird forms of moving creatures, which seem to be floating about lazily in the air, in grottos which glow with a dim light or sparkle with varied colors. One is really looking through glass walls into tanks of water filled with marine life; but both glass and water are so transparent that it is difficult at first glimpse to realize their presence, unless a stream of water, with its attendant bubbles, is playing into the tanks. And even then the effect is most elusive; for the surface of the water, which you are looking up to from below, mirrors the contents of the tanks so perfectly that it is difficult to tell where the reality ends and the image begins, were it not that the duplicated creatures move about with their backs downward in a scene all topsy-turvy. The effect is most fantastic.

More than that, it is most beautiful as well. You are, in effect, at the bottom of the ocean—or rather, at the bottom of many oceans in one. No light comes to you except through the grottos about you—grottos haunted by weird forms of the deep, from graceful to grotesque, from almost colorless to gaudy-hued. To your dilated pupils the light itself has the weird glow of unreality. It is all like the wonders of the *Arabian Nights* made tangible or like a strange spectacular dream. If one were in a great diving-bell at the bottom of the veritable ocean he could hardly feel more detached from the ordinary aerial world of fact.

As one recovers his senses and begins to take definite note of things about him he sees that each one of the many grottos has a different set of occupants, and that not all of the creatures there are as unfamiliar as at first they seemed. Many of the fishes, for example, and the lobsters, crabs, and the like, are familiar enough under other conditions, but even these old acquaintances look strange under these changed circumstances. But for the rest there are multitudes of forms that one had never seen or imagined, for the sea hides a myriad of wonders which we who sail over its surface, and at most glance dimly a few feet into its depths, hardly dream of. Even though one has seen these strange creatures "preserved" in museums, he does not know them, for the alleged preservation there has retained little enough of essential facies of the real creature, which the dead shell can no more than vaguely suggest.

Here, however, we see the real thing. Each creature lives and moves in a habitat as nearly as may be like that which it haunted when at liberty, save that tribes that live at enmity with one another are here separated, so that the active struggle for existence, which plays so large a part in the wild life of sea as well as land, is not represented. For the rest the creatures of the deep are at home in these artificial grottos, and disport themselves as if they desired no other residence. For the most part they pay no heed whatever to the human inspectors without their homelike prisons, so one may watch their activities under the most favorable conditions.

It is odd to notice how curiously sinuous are all the

movements, not alone of the fish, but of a large proportion of the other forms of moving life of the waters. The curve, the line of beauty, is the symbol of their every act; there are no angles in their world. They glide hither and yon, seemingly without an effort, and always with wavy, oscillating gracefulness. The acme of this sinuosity of movement is reached with those long-drawn-out fishes the eels. Of these there are two gigantic species represented here—the conger, a dark-skinned, rather ill-favored fellow, and the beautiful Italian eel, with a velvety, leopard-spotted skin. These creatures are gracefulness itself. They are ribbon-like in tenuousness, and to casual glance they give the impression of long, narrow pennants softly waving in a gentle breeze. The great conger — five or six feet in length—has, indeed, a certain propensity to extend himself rigidly in a fishlike line and lie immovable, but the other species is always true to his colors, so to say — his form is always outlined in curves.

The eels attract their full share of attention from the visitors, but there is one family of creatures which easily holds the palm over all the others in this regard. These are the various representatives of the great cult of squids and cuttle-fishes. The cuttle-fish proper—who, of course, is no fish at all—is shaped strangely like a diminutive elephant, with a filmy, waving membrane along its sides in lieu of legs. Like the other members of his clan, he can change his color variously. Sometimes he is of a dull brown, again prettily mottled; then, with almost kaleidoscopic suddenness, he will assume a garb beautifully striped in black and white, rivalled

THE AQUARIUM OF THE MARINE BIOLOGICAL LABORATORY

by nothing but the coat of the zebra. The cuttle-fish is a sluggish creature, seeking out the darker corners of his grotto, and often lying motionless for long periods together. But not so the little squid. He does not thrive in captivity, and incessantly wings his way back and forth, with slow, wavy flappings of his filmy appendages, until he wears himself out and dies unreconciled.

In marked contrast with both cuttle-fish and squid is their cousin the octopus—a creepy, crawly creature, like eight serpents in one—at once the oddest and the most fascinating creature in the entire aquarium. You will find a crowd almost always before his grotto watching his curious antics. Usually slow and deliberate in movement, he yet has capacity for a certain agility. Now and again he dives off suddenly, head first, through the water, with the directness if not quite with the speed of an arrow. A moment later, tired of his flight, he sprawls his eight webbed legs out in every direction, breaking them seemingly into a thousand joints, and settles back like an animated parachute awreck. Then perchance he perches on a rock knowingly, with the appearance of owl-like wisdom, albeit his head looks surprisingly like a frog's. Anon he holds his head erect and stretches out his long arms in what is most palpably a yawn. Then, for pure diversion, he may hold himself half erect on his umbrella frame of legs and sidle along a sort of quadrille — a veritable "eight hands in round."

But all the while he conveys distinctly the impression of a creature to the last degree blasé. Even when a

crab is let down into his grotto by an attendant for the edification of the visitors the octopus seems to regard it with only lukewarm interest. If he deigns to go in pursuit, it is with the air of one who says, "Anything to oblige," rather than of eagerness for a morsel of food. Yet withal, even though unhurried, he usually falls upon the victim with surprising sureness of aim, encompassing it in his multiform net. Or perhaps, thinking the game hardly worth so much effort, he merely reaches out suddenly with one of his eight arms—each of which is a long-drawn-out hand as well—and grasps the victim and conveys it to his distensible maw without so much as changing his attitude.

All this of the giant octopus—brown and warty and wrinkled and blasé. But the diminutive cousin in the grotto with the jellyfishes is a bird of quite another feather. Physically he is constructed on the same model as the other, but his mentality is utterly opposed. No grand rôles for him; his part is comedy. He finds life full of interest. He is satisfied with himself and with the world. He assumes an aspect of positive rakishness, and intelligence, so to say, beams from his every limb. All day long he must be up and doing. For want of better business he will pursue a shrimp for hours at a time with the zest of a true sportsman. Now he darts after his intended prey like a fox-hound. Again he resorts to finesse, and sidles off, with eyes fixed in another direction, like a master of stratagem. To be sure, he never catches the shrimp—but what of that? The true sportsman is far removed from the necessity for mere material profit. I half suspect that little octopus would release the shrimp if once he caught him,

as the true fisherman throws back the excess of his catch. It is sport, not game, that he covets.

THE LABORATORY AND ITS FOUNDER

When one has made the circuit of the aquarium he will have seen and marvelled at some hundreds of curious creatures utterly unlike anything to be found above water. Brightly colored starfishes, beautiful sea-urchins, strange stationary ascidians, and flower-like sea-anemones, quaint sea-horses, and filmy, fragile jellyfishes and their multiform kin—all seem novel and wonderful as one sees them in their native element. Things that appear to be parts of the rocky or sandy bed of the grottos startle one by moving about, and thus discovering themselves as living creatures, simulating their environment for purposes of protection. Or perhaps what seems to be a giant snail suddenly unfurls wings from its seeming shell, and goes waving through the water, to the utter bewilderment of the beholder. Such freaks as this are quite the rule among the strange tribes of the deep, for the crowding of population there makes the struggle for existence keen, and necessitates all manner of subterfuges for the preservation of species.

Each and every one of the thirty-odd grottos will repay long observation, even on the part of the most casual visitor, and when one has seen them all, he will know more at first hand of the method of life of the creatures of the sea than all the books could teach him. He will depart fully satisfied, and probably, if he be the usual sight-seer, he will never suspect that what he has seen is really but an incidental part of the institution

whose building he has entered. Even though he note casually the inscription "Stazione Zoölogica" above the entrance, he may never suspect that the aquarium he has just visited is only an adjunct—the popular exhibit, so to speak—of the famous institution of technical science known to the English-speaking world as the Marine Biological Laboratory at Naples. Yet such is the fact. The aquarium seems worthy enough to exist by and for itself. It is a great popular educator as well as amuser, yet its importance is utterly insignificant compared with the technical features of the institution of which it is an adjunct.

This technical department, the biological laboratory proper, has its local habitation in the parts of the building not occupied by the aquarium—parts of which the general public, as a rule, sees nothing. There is, indeed, little to see that would greatly interest the casual inspector, for in its outward aspects one laboratory is much like another, a seeming hodgepodge of water-tanks, glass jars of specimens, and tables for microscopes. The real status of a laboratory is not determined by the equipment.

And yet it will not do to press this assertion too far, for in one sense it is the equipment of the Naples laboratory that has made it what it is. Not, however, the equipment in the sense of microscopes and other working paraphernalia. These, of course, are the best of their kind, but machinery alone does not make a great institution, any more than clothes make the man. The all-essential and distinctive equipment of the laboratory reveals itself in its personnel. In the present case, as always in a truly great institution of

any kind, there is one dominating personality, one moving spirit. This is Dr. Anton Dohrn, founder of the laboratory, and still its controller and director, in name and in fact.

More than twenty-five years ago Dr. Dohrn, then a young man fresh from the universities of his native Germany, discovered what he felt to be a real need in the biological world. He was struck with the fact that nowhere in the world could be found an establishment affording good opportunities for the study of marine life. Water covers three-fifths of the earth's surface, as everybody knows, and everywhere this water teems with life, so that a vast preponderance of the living things of the globe find their habitat there. Yet the student who might desire to make special studies of this life would find himself balked at the threshold for want of opportunity.

It was no great thing to discover this paucity, which, indeed, fairly beckoned the discoverer. The great thing was to supply the deficiency, and this was what Dr. Dohrn determined to do. He selected Naples as the best location for the laboratory he proposed to found, because of its climate and its location beside the teeming waters of the Mediterranean. He organized a laboratory; he called about him a corps of able assistants; he made the Marine Biological Laboratory at Naples famous, the Mecca of all biological eyes throughout the world. It was not all done in a day. It was far enough from being done without opposition and discouragement; but these are matters of history which Dr. Dohrn now prefers not to dwell upon. Suffice it that the result aimed at was finally achieved,

and in far greater measure than could at first be hoped for.

And from that day till this Naples has been the centre of that branch of biological inquiry which has for its object the investigation of problems best studied with material gathered from the sea. And this, let me hasten to add, includes far more than a mere study of the life histories of marine animals and plants as such. It includes problems of cell activity, problems of heredity, life problems of many kinds, having far wider horizons than the mere question as to how a certain fish or crustacean lives and moves and has its being.

Dr. Dohrn's chief technical associates are all Germans, like their leader, but, like him also, all gifted with a polyglot mastery of tongues that has stood them in good stead in their intercourse with the biologists of many nationalities who came to work at the laboratory. I must not pause to dwell upon the personnel of the staff in general, but there is one other member who cannot be overlooked even in the most casual survey of the work of the institution. One might almost as well forget Dr. Dohrn himself as to overlook Signor Lo Bianco, chief of the collecting department. Signor Bianco it is who, having expert knowledge of the haunts and habits of every manner of marine creature, can direct his fishermen where to find and how to secure whatever rare specimen any worker at the laboratory may desire. He it is, too, who, by studying old methods and inventing new ones, has learned how to preserve the delicate forms for subsequent study in life-like ensemble that no one else can quite equal. Signor Bianco it is, in short, who is the indispensable right-

hand man of the institution in all that pertains to its practical working outside the range of the microscope.

Each night Signor Lo Bianco directs his band of fishermen as to what particular specimens are most to be sought after next day to meet the needs of the workers in the laboratory. Before sunrise each day, weather permitting, the little scattered fleet of boats is far out on the Bay of Naples; for the surface collecting, which furnishes a large share of the best material, can be done only at dawn, as the greater part of the creatures thus secured sink into the retirement of the depths during the day, coming to the surface to feed only at night. You are not likely to see the collecting party start out, therefore, but if you choose you may see them return about nine or ten o'clock by going to the dock not far from the laboratory. The boats come in singly at about this hour, their occupants standing up to row, and pushing forward with the oars, after the awkward Neapolitan fashion. Many of the fishermen are quaint enough in appearance; some of them have grown old in the service of the laboratory. The morning's catch is contained in glass jars placed in baskets especially constructed for the purpose. The baskets have handles, but these are quite superfluous except to lift them from the boats, for in the transit to the laboratory the baskets are carried, as almost everything else is carried in Naples, on the head. To the novitiate it seems a striking risk to pile baskets of fragile glass and even more fragile specimens one above another, and attempt to balance the whole on the head, but nothing could be easier, or seemingly more secure, for these experts. Arrived at the laboratory, the jars are turned over to

Signor Lo Bianco and his assistants, who sort the material, and send to each investigator in the workrooms whatever he may have asked for.

Of course surface-skimming is not the only method of securing material for the laboratory. The institution owns a steam-launch named the *Johannes Müller*, in honor of the great physiologist, which operates a powerful dredge for securing all manner of specimens from the sea-bottom. Then ordinary lines and nets are more or less in requisition for capturing fish. And in addition to the regular corps of collectors, every fisherman of the neighborhood has long since learned to bring to the laboratory all rare specimens of any kind that he may chance to capture. So in one way and another the institution makes sure of having in tribute all that the richly peopled waters of the Mediterranean can offer. And this well-regulated system of collecting, combined with the richness of the fauna and flora of the Bay of Naples, has no small share in the success of the marine laboratory. But these, of course, were factors that Dr. Dohrn took into account from the beginning.

Indeed, it was precisely with an eye to these important factors that Naples was selected as the site of the future laboratory in the days when the project was forming.

The Bay of Naples is most happily located for the needs of the zoologist. It is not too far south to exclude the fauna of the temperate zone, yet far enough south to furnish a habitat for many forms of life almost tropical in character. It has, in short, a most varied and abundant fauna. And, on the other hand,

the large colony of Neapolitan fishermen made it certain that skilled collectors would always be at hand to make available the wealth of material. It requires no technical education to appreciate the value of this to the original investigator, particularly to the student of life problems. A skilful worker may do much with a single specimen, as, for example, Johannes Müller did half a century ago with the one available specimen of amphioxus, the lowest of vertebrates, then recently discovered. What Müller learned from that one specimen seems almost miraculous. But what if he had had a bucketful of the little boneless creatures at his disposal, as the worker at Naples now may have any day for the asking?

When it comes to problems of development, of heredity, a profusion of material is almost a necessity. But here the creatures of the sea respond to the call with amazing proficiency. Most of them are, of course, oviparous, and it is quite the rule for them to deposit their eggs by hundreds of thousands, by millions even. Everybody knows, since Darwin taught us, that the average number of offspring of any given species of animal or plant bears an inverse proportion to the liability of that species to juvenile fatalities. When, therefore, we find a fish or a lobster or other pelagic creature depositing innumerable eggs, we may feel perfectly sure that the vast majority of the eggs themselves, or the callow creatures that come out of them, will furnish food for their neighbors at an early day. It is an unkind world into which the resident of the deep is born. But his adversity is his human contemporary's gain, and the biologist will hardly be blamed,

even by the most tender-hearted anti-vivisectionist, for availing himself freely of material which otherwise would probably serve no better purpose than to appease the appetite of some rapacious fish.

Their abundance is not the only merit, however, of the eggs of pelagic creatures, in the eyes of the biologist. By equal good-fortune it chances that colorless things are at a premium in the sea, since to escape the eye of your enemy is a prime consideration. So the eggs in question are usually transparent, and thus, shielded from the vision of marine enemies, are beautifully adapted for the observation of the biologist. As a final merit, they are mostly of convenient size for manipulation under the microscope. For many reasons, then, the marine egg offers incomparable advantages to the student of cell life, an egg being the typical cell. And since nowadays the cell is the very focus of attention in the biological world, the importance of marine laboratories has been enhanced proportionately.

But of course not all the work can be done with eggs or with living specimens of any kind. It is equally important on occasion to examine the tissues of adult specimens, and for this, as a rule, the tissues must first be subjected to some preserving and hardening process preliminary to the cutting of sections for microscopical examination. This is done simply enough in the case of some organisms, but there is a large class of filmy, tenuous, fragile creatures in the sea population of which the jellyfish may be mentioned as familiar examples. Such creatures, when treated in an ordinary way, by dropping them into alcohol, shrivel up, coming to resemble nothing in particular, and ceasing to have

any value for the study of normal structures. How to overcome this difficulty was one of the problems attacked from the beginning at the Naples laboratory. The chief part of the practical work of these experiments fell to the share of Signor Lo Bianco. The success that attended his efforts is remarkable. To-day you may see at the laboratory all manner of filmy, diaphanous creatures preserved in alcohol, retaining every jot of their natural contour, and thus offering unexampled opportunities for study *en masse*, or for being sectioned for the microscope. The methods by which this surprising result has been accomplished are naturally different for different creatures; Signor Lo Bianco has written a book telling how it all has been done. Perhaps the most important principle involved with a majority of the more tenuous forms is to stupefy the animal by gradually adding small quantities of a drug, such as chloral, to the water in which the creature is detained. When by this means the animal has been rendered so insensible that it responds very sluggishly to stimuli, it is plunged into a toxic solution, usually formaline, which kills it so suddenly that its muscles in their benumbed state have not time to contract.

Any one who has ever tried to preserve a jellyfish, for example, by ordinary methods will recall the sorry result, and be prepared to appreciate Signor Lo Bianco's wonderfully beautiful specimens. Naturalists have come from all over the world to Naples to learn "just how" the miracle is accomplished, for it must be understood that the mere citation of the *modus operandi* by no means enables the novitiate to apply it successfully at once. In the case of some of the long-

drawn-out forms of clustered ascidians and the like, the delicacy of manipulation required to make successful preservations raises the method as practised at Naples almost to the level of a fine art. It is a boon to naturalists everywhere that the institution here is able sometimes to supply other laboratories less favorably situated with duplicates from its wealth of beautifully preserved specimens.

METHODS AND RESULTS

These, then, are some of the material conditions that have contributed to make the results of the scientific investigations at the Naples laboratory notable. But of course, even with a superabundance of material, discoveries do not make themselves. "Who uses this material?" is, after all, the vital question. And in this regard the laboratory at Naples presents, for any one who gets at its heart, so to speak, an ensemble that is distinctive enough; for the men who work in the light and airy rooms of the laboratory proper have come for the purpose from all corners of the civilized globe, and not a few of them are men of the highest distinction in their various lines of biological science. A large proportion are professors in colleges and universities of their various countries; and for the rest there is scarcely one who is not in some sense master of the biological craft. For it must be understood that this laboratory at Naples is not intended as a training-school for the apprentice. It offers in the widest sense a university course in biology, and that alone. There is no instructor here who shows the new-comer how to use the microscope, how to utilize the material, how to go

about the business of discovery. The worker who comes to Naples is supposed to have learned all these things long before. He is merely asked, then, what class of material he desires, and, this being furnished him, he is permitted to go his own way unmolested. He may work much or little, or not at all; he may make epochal discoveries or no discoveries of any sort, and it will be all one to the management. No one will ask him, in any event, what he has done or why he has not done otherwise. In a word, the worker in the laboratory here, while being supplied with opportunities for study such as he could hardly find elsewhere, retains all the freedom of his own private laboratory.

Little wonder, then, that it is regarded as a rare privilege to be allowed to work in this laboratory. Fortunately, however, it is a privilege that may be obtained by almost any earnest worker who, having learned the technique of the craft elsewhere, desires now to prosecute special original studies in biology. Most of the tables here are leased in perpetuity, for a fixed sum per annum, by various public or private institutions of different countries. Thus, for example, America has the right of use of several tables, the Smithsonian Institution leasing one, Columbia University another, a woman's league a third, and so on. Any American desiring to work at Naples should make application to one of these various sources, stating the exact time when he would like to go, and if there be a vacancy for that time the properly accredited applicant is almost sure to receive the privilege he asks for. Failing in this, however, there is still a court of last appeal in Dr. Dohrn himself, who may have a few

unoccupied tables at his disposal, and who will surely extend the courtesy of their occupancy, for a reasonable period, to any proper applicant, come he whence he may.

Thus it chances that one finds men of all nations working in the Naples laboratory—biologists from all over Europe, including Russia, from America, from Australia, from Japan. One finds women also, but these, I believe, are usually from America. Biologists who at home are at the head of fully equipped laboratories come here to profit by the wealth of material, as well as to keep an eye upon the newest methods of their craft, and to gain the inspiration of contact with other workers in allied fields. Many of the German university teachers, for example, make regular pilgrimages to Naples during their vacations, and more than one of them have made the original investigations here that have given them an international reputation.

As to the exact methods of study employed by the individual workers here, little need be said. In this regard, as in regard to instrumental equipment, one biological laboratory is necessarily much like another, and the general conditions of original scientific experiment are pretty much the same everywhere. What is needed is, first, an appreciation of the logical bearings of the problem to be solved; and, secondly, the skill and patience to carry out long lines of experiments, many of which necessarily lead to no tangible result. The selection of material for the experiments planned, the watching and cultivating of the living forms in the laboratory tanks, the cutting of numberless filmy sections for microscopical examination—these things,

variously modified for each case, make up the work of the laboratory student of general biology. And just in proportion as the experiments are logically planned and carefully executed will the results be valuable, even though they be but negative. Just in proportion as the worker, by inclusion and exclusion, attains authentic results—results that will bear the test of repetition—does his reputation as a dependable working biologist become established.

The subjects attacked in the marine laboratory first and last are practically coextensive with the range of general biology, bacteriology excepted. Naturally enough, the life histories of marine forms of animals and plants have come in for a full share of attention. But, as I have already intimated, this zoological work forms only a small part of the investigations undertaken here, for in the main the workers prefer to attack those general biological problems which in their broader outlines apply to all forms of living beings, from highest to lowest. For example, Dr. Driesch, the well-known Leipzig biologist, spends several months of each year at the laboratory, and has made here most of those studies of cell activities with which his name is associated. The past season he has studied an interesting and important problem of heredity, endeavoring to ascertain the respective shares of the male and female parents in the development of the offspring. The subjects of his experiments have been various species of sea-urchins, but the principles discovered will doubtless be found to apply to most, or perhaps all, forms of vertebrate life as well.

While these studies were under way another devel-

opmental problem was being attacked in a neighboring room of the laboratory by Professor Kitasato, of the University of Tokio, Japan. The subjects this time were the embryos of certain fishes, and the investigation had to do with the development of instructive monstrosities through carefully designed series of injuries inflicted upon the embryo at various stages of its development. Meantime another stage of the developmental history of organic things—this time a microscopical detail regarding the cell divisions of certain plants—has been studied by Professor Mottier, of Indiana; while another American botanist, Professor Swingle, of the Smithsonian Institution, has been going so far afield from marine subjects as to investigate the very practical subject of the fertilization of figs as practised by the agriculturists about Naples.

Even from these few citations it will appear how varied are the lines of attack of a single biological problem; for here we see, at the hands of a few workers, a great variety of forms of life—radiates, insects, vertebrates, low marine plants and high terrestrial ones—made to contribute to the elucidation of various phases of one general topic, the all-important subject of heredity. All these studies are conducted in absolute independence, and to casual inspection they might seem to have little affinity with one another; yet in reality they all trench upon the same territory, and each in its own way tends to throw light upon a topic which, in some of its phases, is of the utmost practical importance to the human family. It is a long vault from the embryo of an obscure sea-weed to the well-being of man, yet it may well happen—so wide in their application

are the general life principles—that study of the one may point a practical moral for the other.

Indeed, it constantly happens that the student of biology, while gazing through his microscope, hits upon discoveries that have the most far-removed implications. Thus a few years ago it was discovered that when a cell is about to bisect itself and become two cells, its nucleus undergoes a curious transformation. Within the nuclear substance little bodies are developed, usually threadlike in form, which take on a deep stain, and which the biologist calls chromosomes. These chromosomes vary in number in the cells of different animals, but the number is always the same for any given species of animal. If one were to group animate beings in classes according to this very fundamental quality of the cells he would have some very curious relations established. Thus, under the heading "creatures whose cells have twenty-four chromosomes," one would find beings so different as "the mouse, the salamander, the trout, and the lily," while the sixteen-chromosome group would introduce the very startling association of the ox, the guinea-pig, the onion, and man himself. But whatever their number, the chromosomes are always exactly bisected before the cell divides, one-half being apportioned to each of the two cells resulting from the division.

Now the application is this: It was the study of these odd nuclear structures and their peculiar manœuvrings that, in large measure, led Professor Weismann to his well-known theory of heredity, according to which the acquired traits of any being are not trans-

missible to the offspring. Professor Weismann came to believe that the apportionment of the nuclear substance, though quantitatively impartial, is sometimes radically uneven in quality; in particular, that the first bisection of the egg-cell, which marks the beginning of embryonic development, produces two cells utterly different in potentiality, the one containing the "body plasm," which is to develop the main animal structures, the other encompassing the "germ plasm," by which the racial integrity is to be preserved. Throughout the life of the individual, he believed, this isolation continued; hence the assumed lack of influence of acquired bodily traits upon the germ plasm and its engendered offspring. Hence, also, the application of the microscopical discovery to the deepest questions of human social evolution.

Every one will recall that this theory, born of the laboratory, made a tremendous commotion in the outside world. Its application to the welfare and progress of humanity gave it supreme interest, and polemics unnumbered were launched in its favor and in its condemnation. Eager search was made throughout the fields of botany and zoology for new evidence pro or con. But the definitive answer came finally from the same field of exploration in which the theory had been originated—the world of the cell—and the Marine Biological Laboratory was the seat of the new series of experiments which demonstrated the untenability of the Weismannian position. Most curious experiments they were, for in effect they consisted of the making of two or more living creatures out of one, in the case of beings so highly organized as the sea-urchins,

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the little fishlike vertebrate, amphioxus, and even the lower orders of true fishes. Of course the division of one being to form two is perfectly familiar in the case of those lowly, single-celled creatures such as the protozoa and the bacteria, but it seems quite another matter when one thinks of cutting a fish in two and having two complete living fish remaining. Yet this is virtually what the biologists did.

Let me hasten to add that the miraculous feat was not accomplished with an adult fish. On the contrary, it is found necessary to take the subject quite at the beginning of its career, when it consists of an egg-cell in the earliest stages of proliferation. Yet the principle is quite the same, for the adult organism is, after all, nothing more than an aggregation of cells resulting from repeated divisions (growth accompanying) and redivisions of that original egg-cell. Considering its potentialities, the egg-cell, seemingly, is as much entitled to be considered an individual as is the developed organism. Yet it transpires that the biologist has been able so to manipulate a developing egg-cell, after its bisection, that the two halves fall apart, and that each half (now become an independent cell) develops into a complete individual, instead of the half-individual for which it seemed destined. A strange trick, that, to play with an individual *Ego*, is it not? The traditional hydra with its reanimating heads was nothing to this scientific hydra, which, when bisected bodily, rises up calmly as two whole bodies.

But even this is not the full measure of the achievement, for it has been found that in some cases the experiment may be delayed until the developing egg

has made a second bisection, thus reaching the four-cell stage, when four completely formed individuals emerge from the dismembered egg. And in the case of certain medusæ, success has attended experiments made at the eight-cell and even at the sixteen-cell stage of development, the creature which had got thus far on its career in single blessedness becoming eight or sixteen individuals at the wave of the enchanted wand—that is to say, the dissecting-needle—of the biologist. All of which savors of conjury, but is really only matter-of-fact biological experiment — experiment, however, of which the implications by no means confine themselves to matters of fact biological. For clearly the fact that the separated egg-cells grow into complete individuals shows that Weismann's theory, according to which one of the cells contained only body plasm, the other only germ plasm, is quite untenable. Thus the theory of the non-transmissibility of acquired characters is deprived of its supposed anatomical support and left quite in the air, to the imminent peril of a school of sociologists who had built thereon new theories of human progress. Also the question of the multiplied personalities clearly extends far beyond the field of the biologist, and must be turned over to the consideration of the psychologist—if, indeed, it does not fall rather within the scope of the moralist.

But though it thus often chances that the biologist, while gazing stoically through his microscope, may discover things in his microcosm that bear very closely upon the practical interests of the most unscientific members of the human family, it would be a mistake to suppose that it is this class of facts that the worker

is particularly seeking. The truth is that, as a rule, the pure biologist is engaged in work for the love of it, and nothing is further from his thoughts than the "practical" bearings or remote implications of what he may discover. Indeed, many of his most hotly pursued problems seem utterly divorced from what an outsider would call practical bearings, though, to be sure, one can never tell just what any new path may lead to. Such, for example, is the problem which, next to questions of cell activities, comes in for perhaps as large a share of attention nowadays as any other one biological topic—namely, the question as to just which of the various orders of invertebrate creatures is the type from which vertebrates were evolved in the past ages—in other words, what invertebrate creature was the direct ancestor of the vertebrates, including man. Clearly it can be of very little practical importance to man of to-day as to just who was his ancestor of several million years ago. But just as clearly the question has interest, and even the layman can understand something of the enthusiasm with which the specialist attacks it.

As yet, it must be admitted, the question is not decisively answered, several rival theories contending for supremacy in the case. One of the most important of these theories had its origin at the Naples laboratory; indeed, Dr. Dohrn himself is its author. This is the view that the type of the invertebrate ancestor is the annelid—a form whose most familiar representative is the earth-worm. The many arguments for and against accepting the credentials of this unaristocratic ancestor cannot be dwelt upon here. But it may be

consolatory, in view of the very plebeian character of the earth-worm, to know that various of the annelids of the sea have a much more aristocratic bearing. Thus the filmy and delicately beautiful structures that decorate the pleasant home of the quaint little sea-horse in the aquarium—structures having more the appearance of miniature palm-trees than of animals—are really annelids. One can view Dr. Dohrn's theory with a certain added measure of equanimity after he learns this, for the marine annelids are seen, some of them, to be very beautiful creatures, quite fitted to grace their distinguished offspring should they make good their ancestral claims.

These glimpses will suffice, perhaps, to give at least a general idea of the manner of thing which the worker at the marine laboratory is seeking to discover when he interrogates the material that the sea has given him. In regard to the publication of the results of work done at the Naples laboratory, the same liberal spirit prevails that actuates the conduct of the institution from first to last. What the investigator discovers is regarded as his own intellectual property, and he is absolutely free, so far as the management of this institution is concerned, to choose his own medium in giving it to the world. He may, and often does, prefer to make his announcements in periodicals or books issued in his own country and having no connection whatever with the Naples laboratory. But, on the other hand, his work being sufficiently important, he may, if he so desire, find a publisher in the institution itself, which issues three different series of important publications under the editorship of Professor Mayer.

One of these, entitled *Mittheilungen aus der Zoologische Station zu Neapel*, permits the author to take his choice among four languages — German, English, French, or Italian. It is issued intermittently, as occasion requires. The second set of publications consists of ponderous monographs upon the fauna and flora of the Gulf of Naples. These are beautifully illustrated in color, and sometimes a single volume costs as much as seventeen thousand dollars to issue. Of course only a fraction of that sum is ever recovered through sale of the book. The third publication, called *Zoologischen Jahresbericht*, is a valuable résumé of biological literature of all languages, keeping the worker at the laboratory in touch with the discoveries of investigators elsewhere.

The latter end is attained further by the library of the institution, which is supplied with all the periodicals of interest to the biologist and with a fine assortment of technical books. The library-room, aside from its printed contents, is of interest because of its appropriate mural decorations, and because of the bronze portrait busts of the two patron saints of the institution, Von Baer and Darwin, which look down inspiringly upon the reader.

All in all, then, it would be hard to find a deficiency in the Stazione Zoologica as an instrument of biological discovery. A long list might be cited of the revelations first brought to light within its walls. And yet, as it seems to me, the greatest value of this institution as an educational factor in science—as a biological lever of progress—does not depend so much upon the tangible revelations of fact that have come out of its

laboratories as upon other of its influences. Scientific ideas, like all other forms of human thought, move more or less in shoals. Very rarely does a great discovery emanate from an isolated observer. The man who cannot come in contact with other workers in kindred lines becomes more or less insular, narrow, and unfitted for progress. Nowadays, of course, the free communication between different quarters of the globe takes away somewhat from the insularity of any quarter, and each scientist everywhere knows something of what the others are doing, through wide-spread publications. But this can never altogether take the place of personal contact and the inspirational communication from man to man. Hence it is that a rendezvous, where all the men of a craft go from time to time and meet their fellows from all over the world, has an influence for the advancement of the guild which is enormous and unequivocal, even though difficult of direct demonstration.

This feature, then, it seems to me, gives Dr. Dohrn's laboratory its greatest value as an educational factor, as a moving force in the biological world. It is true that the new-comer there is likely to be struck at first with a sense of isolation, and to wonder at the seeming exclusiveness of the workers, the self-absorption of each and every one. Outside the management, whom he meets necessarily, no one pays the slightest attention to him at first, or seems to be aware of his existence. He is simply assigned to a room or table, told to ask for what he wants, and left to his own devices. As he walks along the hallways he sees tacked on the doors the cards of biologists from all over the world,

exposing names with which he has long been familiar. He understands that the bearers of the names are at work within the designated rooms, but no one offers to introduce him to them, and for some time, perhaps, he does not so much as see them, nor would he recognize them if he did. He feels strange and isolated in the midst of this stronghold of his profession.

But soon this feeling leaves him. He begins to meet his fellow-workers casually here and there—in the hallways, at the distributing-tanks, in the library. There are no formal gatherings, and there are some workers who never seem to affiliate at all with the others; but in the long-run, here as elsewhere, kindred spirits find one another out; and even the unsocial ones take their share, whether or no, in the indefinable but very sensible influence of massed numbers. Presently some one suggests to the new-comer that he join some of the others of a Wednesday or Saturday evening, at a rendezvous where a number of them meet regularly. He goes, under escort of his sponsor, and is guided through one of those narrow, dark, hill-side streets of Naples where he would hardly feel secure to go alone, to a little wine-shop in what seems a veritable dungeon—a place which, if a stranger in Naples, he would never even remotely think of entering. But there he finds his confrères of the laboratory gathered about a long table, with the most conglomerate groups of Neapolitans of a seemingly doubtful class at their elbows. Each biologist has a caraffa of light wine on the table before him, and all are smoking. And, staid men of science that they are, they are chattering away on trivial topics with the animation of a company of

school-boys. The stock language is probably German, for this bohemian gathering is essentially a German institution; but the Germans are polyglots, and you will hardly find yourself lost in their company, whatever your native tongue.

Your companions will tell you that for years the laboratory fraternity have met twice a week at this homely but hospitable establishment. The host, honest Dominico Vincenzo Bifulco, will gladly corroborate the statement by bringing out for inspection a great blank-book in which successive companies of his guests from the laboratory have scrawled their names, written epigrams, or made clever sketches. That book will some day be treasured in the library of a bibliophile, but that will not be until Bifulco is dead, for while he lives he will never part with it.

One comes to look upon this bohemian wine-shop as an adjunct of the laboratory, and to feel that the free-and-easy meetings there are in their way as important for the progress of science as the private séances of the individual workers in the laboratory itself. Not because scientific topics are discussed here, though doubtless that sometimes happens, but because of that vitalizing influence of the contact of kindred spirits of which I am speaking, and because this is the one place where a considerable number of the workers at the laboratory meet together with regularity.

The men who enter into such associations go out from them revitalized, full of the spirit of propaganda. Returned to their own homes, they agitate the question of organizing marine laboratories there; and it is largely through the efforts of the graduates, so to say,

of the Naples laboratory that similar institutions have been established all over the world.

Thanks largely to the original efforts of Dr. Dohrn, nearly all civilized countries with a coast-line now have their marine laboratories. France has half a dozen, two of them under government control. Russia has two on the Black Sea and one on the French Mediterranean coast. Great Britain has important stations at St. Andrews, at Liverpool, and at Plymouth. The Scandinavian peninsula has also three important stations. Germany shows a paucity by comparison, which, however, is easily understood when one reflects that the mother-laboratory at Naples is essentially a German institution despite its location.

The American stations are located at Woods' Holl and at Cold Spring Harbor, on opposite coasts of Long Island Sound. The Japanese station is an adjunct of Tokio University. For the rest, the minor offspring of the Naples laboratory are too numerous to be cited here. Nor can I enter into any details regarding even the more important ones. Each in its way enters into the same general line of work, varying the details according to the bent of mind of individual directors and the limitations of individual resources. But in the broader outlines the aim of all is the same, and what we have seen at Naples is typical of what is best in all the others.

VI

ERNST HAECKEL AND THE NEW ZOOLOGY

THE DREAM CITY

THE train crept on its tortuous way down the picturesque valley of the little Saale. At last we saw, high above us, on a jutting crag, three quaint old castles, in one of which, as we knew from our *Baedeker*, Goethe at one time lived. We were entering the region of traditions. Soon we knew we should be passing that famous battle-field on which Napoleon, in 1806, sealed the fate of Germany for a generation. But this spot, as seen from the car window, bore no emblem to distinguish it, and before we were quite sure that we had reached it we had in point of fact passed on, and the train was coming to a stop. "Jena!" called the guard, and the scramble for "luggage" began, leaving us for the moment no place for other thoughts than to make sure that all our various parcels were properly dragged out along with ourselves. For a wonder no *Dienstman* appeared to give us aid —showing how unexpected is the arrival of any wayfarer at this untoward season—and for a moment one seemed in danger of being reduced to the unheard-of expedient of carrying one's own satchel. But, fortunately, one is rescued from this most un-German predicament by the porter of a waiting hotel omnibus,

and so at last we have time to look about us, and to awaken to a realizing sense that we have reached the land of traditions; that we have come to Mecca; that we are in the quondam home of Guericke, Fichte, Goethe, Schiller, Oken, and Gagenbaur; in the present home of Haeckel.

The first glimpse of a mountain beaming down at us from across the way was in admirable conformity with our expectations, but for the rest, the vicinage of the depot presented a most distressing air of modernity. A cluster of new buildings—some of them yet unfinished—stared back at us and the mountain with the most barefaced aspect of cosmopolitanism. Was this, then, Jena, the home of traditions? Or were we entering some Iowa village, where the first settlers still live who but yesterday banished the prairie-dog and the buffalo?

But this disappointment and its ironical promptings were but fleeting. Five minutes' drive and we were in the true Jena with the real flavor of mediævalism about us. Here is the hostelry where Luther met the Swiss students in 1522. There is nothing in that date to suggest our Iowa village, nor in the aspect of the hostelry itself, thank fortune. And there rises the spire of the city church, up the hill yonder, which was aging, as were most of the buildings that still flank it, when Luther made that memorable visit. America was not discovered, let alone Iowa, when these structures were erected. Now, sure enough, we are in the dream city.

A dream city it truly seems, when one comes to wander through its narrow, tortuous streets, between

time-stained walls, amid its rustic population. Coming from Berlin, from Dresden, from Leipzig—not to mention America—one feels as if he had stepped suddenly back two or three centuries into the past. There are some evidences of modernity that mar the illusion, to be sure; but the preponderance of the old-time emblems is sufficient to leave the mind in a delightful glow of reminiscences. As a whole, the aspect of the central portion of the village—of the true Jena—cannot greatly have changed since the days when Luther stopped here on his way to Wittenberg; surely not since 1662, when the mighty young Leibnitz, the Aristotle of Germany, came to Jena to study under Weigel, the most famous of German mathematicians of that century. Here and there an old house has been demolished, to be sure; even now you may see the work of destruction going on, as a new street is being cut through a time-honored block close to the old church. But in the main the old thoroughfares run hither and thither, seemingly at random, as of old, disclosing everywhere at their limits a sky-line of picturesque gables, and shut in by walls that often are almost cañon-like in narrowness; while the heavy, buttressed doors and the small, high-placed windows speak of a time when every house partook of the nature of the fortress.

The footway of the thoroughfares has no doubt vastly changed, for it is for the most part paved now—badly enough, to be sure, yet, after all, paved as no city was in the good old days when garbage filled the streets and cleanliness was an unknown virtue. The Jena streets of to-day are very modern in their cleanli-

ness; yet a touch of mediævalism is retained in that the main work of cleaning is done by women. But, for that matter, it seems to the casual observer as if the bulk of all the work here were performed by the supposedly weaker sex. Certainly woman is here the chief beast of burden. In every direction she may be seen, in rustic garb, struggling cheerily along under the burden of a gigantic basket strapped at her back. You may see the like anywhere else in Germany, to be sure, but not often elsewhere in such preponderant numbers. And scarcely elsewhere does the sight jar so little on one's New-World sensibilities as in the midst of this mediæval setting. One is even able to watch the old women sawing and splitting wood in the streets here, with no thought of anything but the picturesqueness of the incident.

If one follows a band of basket-laden women, he will find that their goal is that focal-point of every old-time city, the market-place. There arrived, he will witness a scene common enough in Europe but hardly to be duplicated anywhere in America. Hundreds of venders of meat, fish, vegetables, cloths, and household utensils have their open-air booths scattered all across the wide space, and other hundreds of purchasers are there as well. Quaint garbs and quainter faces are everywhere, and the whole seems quite in keeping with the background of fifteenth-century houses that hedges it in on every side. Could John the Magnanimous, who rises up in bronze in the midst of the assembly, come to life, he would never guess that three and a half centuries have passed since he fell into his last sleep.

This same John the Magnanimous it was who founded the institution which gives Jena its fame and distinguishes it from all the other quaint hypnotic clusters of houses that nestle similarly here and there in other picturesque valleys of the Fatherland — I mean, of course, its world-renowned university. It is but a few minutes' walk from the market-place, past the home where Schiller once lived and through the "street" scarcely more than arms'-breadth wide beyond, to the site of the older buildings of the university. Inornate, prosaic buildings they are, unrelieved even by the dominant note of picturesqueness; rescued, however, from all suggestion of the commonplace by the rugged ruins of the famed "powder-tower" jutting out from the crest of the hill just above, by the spire of the old church which seems to rise from the oldest university building itself, and by the mountain peaks that jut up into view far beyond.

If you would enter one of the old buildings there is naught to hinder. Go into one of the lecture-halls which chances at the moment to be unoccupied, and you will see an array of crude old benches for seats that look as if they might have been placed there at the very inaugural of the institution. The boards that serve for desks, if you scan them closer, you will find scarred all over with the marks of knives, showing how some hundreds of successive classes of listeners have whiled away the weary lecture-hours. Not a square inch can you find of the entire desk surface that is unscarred. If one would woo a new sensation, he has but to seat himself on one of these puritanical old benches and conjure up in imagination the long series

THE MARKET-PLACE, JENA

of professors that may have occupied the raised platform in front, recalling the manner of thought and dogma that each laid down as verity. He of the first series appears in the garb of the sixteenth century, with mind just eagerly striving to peer a little way out of the penumbra of the Renaissance. The students who carve the first gashes in the new desks will learn, if perchance they listen in intervals of whittling, that this world on which they live is perhaps not flat, but actually round, like a ball. It is debatable doctrine, to be sure, but we must not forget that Signor Columbus, recently dead, found land off to the west which is probably a part of the Asiatic continent. If the earth be indeed a ball, then the sun and stars whirl clear around it in twenty-four hours, travelling thus at an astonishing speed, for the sphere in which they are fastened is situated hundreds of miles away. The sun must be a really great ball of fire — perhaps a mile even in diameter. The moon, as is plain to see, is nearly as large. The stars, of course, are only sparks, though of great brilliancy. They are fixed in a different sphere from that of the sun. In still other spheres are the moon, and a small set of large stars called planets, of which latter there are four, in order that, with the sun, the moon, and the other stars, there may be made seven orders of heavenly bodies— seven being, of course, the magic number in accordance with which the universe is planned.

This is, in substance, the whole subject of astronomy, as that first professor must have taught it, even were he the wisest man of his time. Of the other sciences, except an elementary mathematics, there was hardly

so much as an inkling taught that first class of students. You will find it appalling, as you muse, to reflect upon the amazing mixture of utter ignorance and false knowledge which the learned professor of that day brought to the class-room, and which the "educated" student carried away along with his degree. The one and the other knew Greek, Latin, and Bible history and doctrine. Beyond that their minds were as the minds of babes. Yet no doubt the student who went out from the University of Jena in the year 1550 thought himself upon the pinnacles of learning. So he was in his day and age, but could he come to life to-day, in the full flush of his scholarship, yonder wood-vender, plying her saw out here in front of the university building, would laugh in derision at his simplicity and ignorance. So it seems that, after all, the subjects of John the Magnanimous have changed more than a little during the three hundred and odd years that John himself, done in bronze, has been standing out there in the market-place.

THE CAREER OF A ZOOLOGIST

Had one time for it, there would be real interest in noting the steps by which the mental change in question has been brought about; in particular to note the share which the successive generations of Jena professors have taken in the great upward struggle. But we must not pause for that here. Our real concern, despite the haunting reminiscences, is not with the Jena of the past, but with the Jena of to-day; not with ghosts, but with the living personality who has made the Jena of our generation one of the greatest centres

of progress in human thought in all the world. Jena is Jena to-day not so much because Guericke and Fichte and Hegel and Schiller and Oken taught here in the past, as because it has for thirty-eight years been the seat of the labors of Germany's greatest naturalist, one of the most philosophical zoologists of any country or any age, Professor Ernst Haeckel. It is of Professor Haeckel and his work that I chiefly mean to write, and if I have dwelt somewhat upon Jena itself, it is because this quaint, retired village has been the theatre of Haeckel's activities all the mature years of his life, and because the work he has here accomplished could hardly have been done so well elsewhere; some of it, for reasons I shall presently mention, could hardly have been done elsewhere at all—at least in another university.

It was in 1861 that young Dr. Haeckel came first to Jena as a teacher. He had made a tentative effort at the practice of medicine in Berlin, then very gladly had turned from a distasteful pursuit to the field of pure science. His first love, before he took up the study of medicine, had been botany, though pictorial art, then as later, competed with science for his favorable attention. But the influence of his great teacher, Johannes Müller, together with his medical studies, had turned his attention more directly to the animal rather than vegetable life, and when he left medicine it was to turn explicitly to zoology as a life study. Here he believed he should find a wider field than in art, which he loved almost as well, and which, it may be added, he has followed all his life as a dilettante of much more than amateurish skill. Had he so elected, Haeckel might

have made his mark in art quite as definitely as he has made it in science. Indeed, even as the case stands, his draughtsman's skill has been more than a mere recreation to him, for without his beautiful drawings, often made and reproduced in color, his classical monographs on various orders of living creatures would have lacked much of their present value.

Moreover, quite aside from these merely technical drawings, Professor Haeckel has made hundreds of paintings purely for recreation and the love of it, illustrating—and that too often with true artistic feeling for both form and color—the various lands to which his zoological quests have carried him, such as Sicily, the Canaries, Egypt, and India. From India alone, after a four-months' visit, Professor Haeckel brought back two hundred fair-sized water-colors, a feat which speaks at once for his love of art and his amazing industry.

I dwell upon this phase of Professor Haeckel's character and temperament from the very outset because I wish it constantly to be borne in mind, in connection with some of the doctrines to be mentioned presently, that here we have to do with no dry-as-dust scientist, cold and soulless, but with a broad, versatile, imaginative mind, one that links the scientific and the artistic temperaments in rarest measure. Charles Darwin, with whose name the name of Haeckel will always be linked, told with regret that in his later years he had become so steeped in scientific facts that he had lost all love for or appreciation of art or music. There has been no such mental warping and atrophy in the mind of Ernst Haeckel. Yet there is probably no man liv-

ing to-day whose mind contains a larger store of technical scientific facts than his, nor a man who has enriched zoology with a larger number of new data, the result of direct personal observation in field or laboratory.

How large Haeckel's contribution in this last regard has been can be but vaguely appreciated by running over the long list of his important publications, though the list includes more than one hundred titles, unless it is understood that some single titles stand for monographs of gigantic proportions, which have involved years of labor in the production. Thus the text alone of the monograph on the radiolarians, a form of microscopic sea-animalcule (to say nothing of the volume of plates), is a work of three gigantic volumes, weighing, as Professor Haeckel laughingly remarks, some thirty pounds, and representing twelve years of hard labor. This particular monograph, by-the-bye, is written in English (of which, as of several other languages, Professor Haeckel is perfect master), and has a history of more than ordinary interest. It appears that the radiolarians were discovered about a half-century ago by Johannes Müller, who made an especial study of them, which was uncompleted at the time of his death in 1858. His monograph, describing the fifty species then known, was published posthumously. Haeckel, on whom the mantle of the great teacher was to fall, and who had been Müller's last pupil, took up the work his revered master had left unfinished as his own first great original *Arbeit*. He went to Messina and was delighted to find the sea there replete with radiolarians, of which he was able to discover one or two new species

almost every day, until he had added one hundred and fifty all told to Müller's list, or more than triple the whole number previously known. The description of these one hundred and fifty new radiolarians constituted Haeckel's first great contribution to zoology, and won him his place as teacher at Jena in 1861.

Henceforth Haeckel was, of course, known as the greatest authority on this particular order of creatures. For this reason it was that Professor Murray, the naturalist of the famous expedition which the British government sent around the world in the ship *Challenger*, asked Haeckel to work up the radiolarian material that had been gathered during that voyage. Murray showed Haeckel a little bottle containing water, with a deposit of seeming clay or mud in the bottom. "That mud," he said, "was dredged up from the bottom of the ocean, and every particle of it is the shell of a radiolarian." "Impossible," said Haeckel. "Yet true," replied Murray, "as the microscope will soon prove to you."

So it did, and Professor Haeckel spent twelve years examining that mud under the microscope, with the result that, before he had done, he had discovered no fewer than four thousand new species of radiolarians, all of which, of course, had to be figured, described, and christened. Think of baptizing four thousand creatures, finding a new, distinct, and appropriate Latin name for each and every one, and that, too, when the creatures themselves are of microscopic size, and the difference between them often so slight that only the expert eye could detect it. Think, too, of the deadly tedium of labor in detecting these differences, in sketch-

ing them, and in writing out, to the length of three monster volumes, technical dissertations upon them.

To the untechnical reader that must seem a deadly, a veritably mind-sapping task. And such, indeed, it would prove to the average zoologist. But with the mind of a Haeckel it is far otherwise. To him a radiolarian, or any other creature, is of interest, not so much on its own account as for its associations. He sees it not as an individual but as a link in the scale of organic things, as the bearer of a certain message of world-history. Thus the radiolarians, insignificant creatures though they seem, have really taken an extraordinary share in building up the crust of the earth. The ooze at the bottom of the sea, which finally becomes metamorphosed into chalk or stone, is but the aggregation of the shells of dead radiolarians. In the light of such a rôle the animalcule takes on a new interest.

But even greater is the interest that attaches to every creature in regard to the question of its place in the organic scale of evolution. What are the homologies of this form and that? What its probable ancestry? What gaps does it bridge? What can it tell us of the story of animal creation? These and such like are the questions that have been ceaselessly before Haeckel's mind in all his studies of zoology. Hence the rich fountain of philosophical knowledge that has welled up from what otherwise might have been the most barren of laboratory borings. Thus from a careful investigation of the sponge Haeckel was led to his famous gastrula theory, according to which the pouch-like sponge-animalcule—virtually a stomach without members—is the type of organism on which all high

organisms are built, so to speak—that is, out of which all have evolved.

This gastrula theory, now generally accepted, is one of Haeckel's two great fundamental contributions to the evolution philosophy with the history of which his life work is so intimately linked. The other contribution is the theory, even more famous and now equally undisputed, that every individual organism, in its embryological development, rehearses in slurred but unmistakable epitome the steps of evolution by which the ancestors of that individual came into racial being. That is to say, every mammal, for example, originating in an egg stage, when it is comparable to a protozoon, passes through successive stages when it is virtually in succession a gastrula, a fish, and an amphibian before it attains the mammalian status, because its direct ancestors were in succession, through the long geological ages, protozoons, gastrulæ, fishes, amphibians before the true mammal was evolved. This theory cast a flood of light into many dark places of the Darwinian philosophy. It was propounded in 1866 in Professor Haeckel's great work on morphology, and it has ever since been a guiding principle in his important philosophical studies.

It was through this same work on morphology that Haeckel first came to be universally recognized as the great continental champion of Darwinism—the Huxley of Germany. Like Huxley, Haeckel had at once made the logical application of the Darwinian theory to man himself, and he sought now to trace the exact lineage of the human family as no one had hitherto attempted to fathom it. Utilizing his wide range

of zoological and anatomical knowledge, he constructed a hypothetical tree of descent—or, if you prefer, ascent —from the root in a protozoon to the topmost twig or most recent offshoot, man. From that day till this Haeckel's persistent labors have been directed towards the perfection of that genealogical tree.

This work on morphology was much too technical to reach the general public, but in 1868 Haeckel prepared, at the instigation of his friend and confrère Gagenbaur, what was practically a popular abridgment of the technical work, which was published under the title of *The Natural History of Creation.* This work created a furor at once. It has been translated into a dozen languages, and has passed through nine editions in the original German. Through it the name of Haeckel became almost a household word the world over, and subject for mingled applause and opprobrium — applause from the unprejudiced for its great merit; opprobrium from the bigoted because of the unprecedented candor with which it followed the Darwinian hypothesis to its logical goal.

The same complete candor of expression has marked every stage of the unfolding of Professor Haeckel's philosophical pronouncements. This fact is the more remarkable because Professor Haeckel is, so far as I am aware, the only scientist of our generation who has felt at liberty to announce, absolutely without reserve, the full conclusions to which his philosophy has carried him, when these conclusions ran counter to the prevalent prejudices of his time. Some one has said that the German universities are oases of freedom. The remark is absolutely true of Jena. It is not true,

I believe, in anything like the same degree of any other German university, or of any other university in the world. One thing before others that has endeared Jena to Haeckel, and kept him there in the face of repeated flattering calls to other universities, is that full liberty of spirit has been accorded him there, as he knew it would not be accorded elsewhere. "When a man comes into the atmosphere of Jena," says Professor Haeckel, "he perforce begins to think—there is no escape from it. And he is free to let his thoughts carry him whithersoever they honestly may. My beliefs," he added, "are substantially the beliefs of my colleagues in science everywhere, as I know from private conversations; but they, unlike myself, are not free to speak the full truth as they see it. I myself would not be tolerated elsewhere, as I am well aware. Had I desired to remain in Berlin, for example, I must have kept silent. But here in Jena one is free."

And he smiles benignly as he says it. The controversies through which he has passed and the calumnies of which he has been the target have left no scars upon this broad, calm spirit.

HAECKEL AS MAN AND TEACHER

It is indeed a delightful experience to meet Professor Haeckel in the midst of his charming oasis of freedom, his beloved Jena. To reach his laboratory you walk down a narrow lane, past Schiller's house, and the garden where Schiller and Goethe used to sit and where now the new observatory stands. Haeckel's laboratory itself is a simple oblong building of yellowish brick, standing on a jutting point of land high above the

street-level. Entering it, your eye is first caught by a set of simple panels in the wall opposite the door bearing six illustrious names: Aristotle, Linne, Lamarck, Cuvier, Müller, Darwin — a Greek, a Swede, two Frenchmen, a German, and an Englishman. Such a list is significant; it tells of the cosmopolitan spirit that here holds sway.

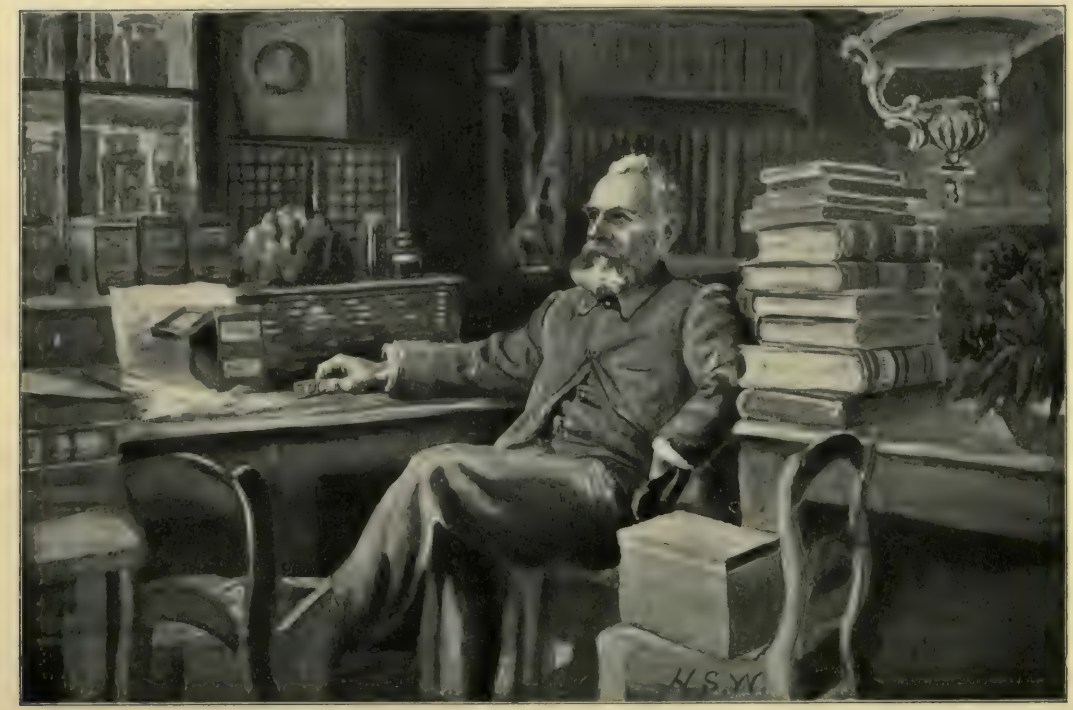
The ground-floor of the building is occupied by a lecture-room and by the zoological collection. The latter is a good working-collection, and purports to be nothing else. Of course it does not for a moment compare with the collections of the museums in any large city of Europe or America, nor indeed is it numerically comparable with many private collections, or collections of lesser colleges in America. Similarly, when one mounts the stairs and enters the laboratory proper, he finds a room of no great dimensions and nowise startling in its appointments. It is admirably lighted, to be sure, and in all respects suitably equipped for its purpose, but it is by no means so large or so luxurious as the average college laboratory of America. Indeed, it is not to be mentioned in the same breath with the laboratories of a score or two of our larger colleges. Yet, with Haeckel here, it is unquestionably the finest laboratory in which to study zoology that exists in the world to-day, or has existed for the last third of a century.

Haeckel himself is domiciled, when not instructing his classes, in a comfortable but plain room across the hall—a room whose windows look out across the valley of the Saale on an exquisite mountain landscape, with the clear-cut mountain that Schiller's lines made fa-

mous at its focus. As you enter the room a big, robust man steps quickly forward to grasp your hand. Six feet or more in height, compactly built, without corpulence; erect, vigorous, even athletic; with florid complexion and clear, laughing, light-blue eyes that belie the white hair and whitening beard; the ensemble personifying at once kindliness and virility, simplicity and depth, above all, frank, fearless honesty, without a trace of pose or affectation—such is Ernst Haeckel. There is something about his simple, frank, earnest, sympathetic, yet robust, masculine personality that reminds one instinctively, as does his facial contour also, of Walt Whitman.

A glance about the room shows you at once that it is a place for study, and also that it is the room of the most methodical of students. There are books and papers everywhere, yet not the slightest trace of disorder. Clearly every book and every parcel of papers has a place, and is kept in that place. The owner can at any moment lay his hand upon anything he desires among all these documents. This habit of orderliness has had no small share, I take it, in contributing to Professor Haeckel's success in carrying forward many lines of research at the same time, and carrying all to successful terminations. Then there goes with it, as a natural accompaniment, a methodical habit of working, without which no single man could have put behind him the multifarious accomplishments that stand to Professor Haeckel's credit.

Orderliness is not a more pronounced innate gift with Professor Haeckel than is the gift of initial energy to undertake and carry on work which leads to accom-



PROFESSOR ERNST HAECKEL IN HIS STUDY

plishment—a trait regarding which men, even active men, so widely differ. But Professor Haeckel holds that whatever his normal bent in this direction, it was enormously strengthened in boyhood by the precepts of his mother—from whom, by-the-bye, he chiefly inherits his talents. "My mother," he says, "would never permit me to be idle for a moment. If I stood at a window day-dreaming, she would always urge me to be up and doing. 'Work or play,' she would urge, 'but do not stand idle.' Through this reiterated admonition, physical activity became a life-long habit with me, and work almost a necessity of my being. If I have been able to accomplish my full share of labors, this is the reason. I am never idle, and I scarcely know the meaning of *ennui*."

This must not be interpreted as meaning, however, that Professor Haeckel takes up a task and works at it all day long unceasingly. That is not the German method of working, and in this regard Professor Haeckel is a thorough German. "When I was a young man," he says, "I at one time, thanks to the persuasions of some English friends, became a convert to the English method of working, and even attempted to introduce it into Germany. But I soon relinquished it, and lapsed back into our German method, which I am convinced will produce better results for the average worker. The essential of this method is the long midday rest, which enables one late in the afternoon to begin what is virtually a new day's-work, and carry it out with vigor and without undue fatigue. Thus I, who am an early riser, begin work at five in summer and six in winter, after the customary light breakfast

of coffee and rolls. I do not take a second breakfast at ten or eleven, as many Germans do, but work continuously until one o'clock, when I have dinner. This, with me, as with all Germans, is the hearty meal of the day. After dinner I perhaps take a half-hour's nap; then read the newspaper, or chat with my family for an hour, and perhaps go for a long walk. At about four, like all Germans, I take my cup of coffee, but without cake or other food. Then, at four, having had three full hours of brain-rest and diversion, I am ready to go to work again, and can accomplish four hours more of work without undue fatigue. At eight I have my rather light supper, and after that I attempt no further work, giving the evening to reading, conversation, or other recreation. I do not retire till rather late, as I require only five or six hours' sleep."

Such is the method of labor division that enables not Professor Haeckel only, but a host of other German brain-workers to accomplish enormous labors, yet to thrive on the accomplishment and to carry the ruggedness and health of youth far into the decades that are too often with our own workers given over to decrepitude. Haeckel at sixty-five looks as if he were good for at least a score of years of further effort. And should he fulfil the promise of his present ruggedness, he will do no more than numbers of his colleagues in German universities have done and are doing. When one runs over the list of octogenarians, and considers at the same time the amount of the individual output of the best German workers, he is led to feel that Professor Haeckel was probably right in

giving up the continuous-day method of labor and reverting to the German method.

In addition to the original researches that Professor Haeckel has carried out, to which I have already made some reference, there has, of course, been all along another large item of time-consumption to be charged up to his duties as a teacher. These, to be sure, are somewhat less exacting in the case of a German university professor than they are in corresponding positions in England or America. Thus, outside the hours of teaching, Professor Haeckel has all along been able to find about eight hours a day for personal, original research. When he told Professor Huxley so in the days of their early friendship, Huxley exclaimed: "Then you ought to be the happiest man alive. Why, I can find at most but two hours a day to use for myself."

So much for the difference between German methods of teaching, where the university professor usually confines his contact with the pupils to an hour's lecture each day, and the English system, according to which the lecturer is a teacher in other ways as well. Yet it must be added that in this regard Professor Haeckel is not an orthodox German, for his contact with his students is by no means confined to the lecture-hour. Indeed, if one would see him at his best, he must go, not to the lecture-hall, but to the laboratory proper during the hours when Professor Haeckel personally presides there, and brings knowledge and inspiration to the eager band of young dissectors who gather there. It will perhaps seem strange to the reader to be told that the hours on which this occurs are from nine

till one o'clock of a day which is perhaps not devoted to class-room exercises in any other school of Christendom whatever—namely, the Sabbath. It is interesting to reflect what would be the comment on such a procedure in London, for example, where the underground railway trains even must stop running during the hours of morning service. But Jena is not London, and, as Professor Haeckel says, "In Jena one is free. It pleases us to have our Sabbath service in our tabernacle of science."

All questions of time aside, it is a favored body of young men who occupy the benches in the laboratory during Professor Haeckel's unique Sunday-morning service. Each student has before him a microscope and a specimen of the particular animal that is the subject of the morning's lesson. Let us say that the subject this morning is the crawfish. Then in addition to the specimens with which the students are provided, and which each will dissect for himself under the professor's guidance, there are scattered about the room, on the various tables, all manner of specimens of allied creatures, such as crabs, lobsters, and the like. There are dissected specimens also of the crawfish, each preparation showing a different set of organs, exhibited in preserving fluids. Then there are charts hung all about the room illustrating on a magnified scale, by diagram and picture, all phases of the anatomy of the subjects under discussion. The entire atmosphere of the place this morning smacks of the crawfish and his allies.

The session begins with a brief off-hand discussion of the general characteristics and affinities of the group

HAECKEL TEACHING IN HIS LABORATORY

of arthropoda, of which the crawfish is a member. Then, perhaps, the professor calls the students about him and gives a demonstration of the curious phenomena of hypnotism as applied to the crawfish, through which a living specimen, when held for a few moments in a constrained attitude, will pass into a rigid "trance," and remain standing on its head or in any other grotesque position for an indefinite period, until aroused by a blow on the table or other shock. Such are some of the little asides, so to speak, with which the virile teacher enlivens his subject and gives it broad, human interest. Now each student turns to his microscope and his individual dissection, and the professor passes from one investigator to another with comment, suggestion, and criticism; answering questions, propounding anatomical enigmas for solution—enlivening, vivifying, inspiring the entire situation.

As the work proceeds, Professor Haeckel now and again calls the attention of the entire class to some particular phase of the subject just passing under their individual observation, and in the most informal of talks, illustrated on blackboard and chart, clears up any lurking mysteries of the anatomy, or enlivens the subject with an incursion into physiology, embryology, or comparative morphology of the parts under observation. Thus by the close of the session the student has something far more than a mere first-hand knowledge of the anatomy of the crawfish — though that in itself were much. He has an insight also into a half-dozen allied subjects. He has learned to look on the crawfish as a link in a living chain—a creature with physiological, psychological, ontological affinities that

give it a human interest not hitherto suspected by the novitiate. And when the entire series of Sunday-morning "services" has been carried through, one order after another of the animal kingdom being similarly made tribute, the favored student has gone far towards the goal of a truly philosophical zoology, as different from the old-time dry-bones anatomy as the living crawfish is different from the dead shell which it casts off in its annual moulting time.

THE NEW ZOOLOGY

What, then, is the essence of this "philosophical zoology," of which Haeckel is the greatest living exponent and teacher and of which his pupils are among the most active promoters? In other words, what is the real status, and the import and meaning, the *raison d'être*, if you will, of the science of zoology to-day?

To clear the ground for an answer to that question, one must glance backward, say half a century, and note the status of the zoology of that day, that one may see how utterly the point of view has changed since then; what a different thing zoology has become in our generation from what it was, for example, when young Haeckel was a student at Jena back in the fifties. At that time the science of zoology was a conglomeration of facts and observations about living things, grouped about a set of specious and sadly mistaken principles. It was held, following Cuvier, that the beings of the animal kingdom had been created in accordance with five preconceived types: the vertebrate, with a spinal column; the articulate, with jointed body and members, as represented by the familiar crustaceans and insects;

the mollusk, of which the oyster and the snail are familiar examples; the radiate, with its axially disposed members, as seen in the starfish; and the low, almost formless protozoon, most of whose representatives are of microscopic size. Each of these so-called classes was supposed to stand utterly isolated from the others, as the embodiment of a distinct and tangible idea. So, too, of the lesser groups or orders within each class, and of the still more subordinate groups, named technically families, genera; and, finally, the individual species. That the grouping of species into these groups was more or less arbitrary was of course to some extent understood, yet it was not questioned by the general run of zoologists that a genus, for example, represented a truly natural group of species that had been created as variations upon one idea or plan, much as an architect might make a variety of houses, no one exactly like any other, yet all conforming to a particular type or genus of architecture—for example, the Gothic or the Romanesque. That each of the groups defined by the classifiers had such status as this was the stock doctrine of zoology, as also that the individual species making up the groups, and hence the groups themselves, maintained their individual identity absolutely unaltered from the moment of their creation, throughout all successive generations, to the end of their racial existence.

Such being the fundamental conception of zoology, it remained only for the investigator to study each individual species with an eye to its affinities with other species, that each might be assigned by a scientific classification to the particular place in the original

scheme of creation which it was destined to occupy. Once such affinities had been correctly determined and interpreted for all species, the zoological classification would be complete for all time. A survey of the completed schedule of classification would then show at a glance the details of the preconceived system in accordance with which the members of the animal kingdom were created, and zoology would be a "finished" science.

In the application of this relatively simple scheme, to be sure, no end of difficulties were encountered. Each higher animal is composed of so many members and organs, of such diverse variations, that naturalists could never agree among themselves as to just where a balance of affinities between resemblances and differences should be struck; whether, for example, a given species varied so much from the type species of a genus —say the genus Gothic house—as to belong properly to an independent genus — say Romanesque house; or whether, on the other hand, its divergencies were still so outweighed by its resemblances as to permit of its retention as an aberrant member of genus number one. Perpetual quibbling over these matters was quite the order of the day, no two authorities ever agreeing as to details of classification. The sole point of agreement was that preconceived types were in question—if only the zoologists could ever determine just what these types were. Meantime, the student who supposed classifications to be matters of moment, and who laboriously learned to label the animals and birds of his acquaintance with an authoritative Latin name, was perpetually obliged to unlearn what he had

acquired, as a new classifier brought new resources of hair-splitting pursuit of a supposed type or ideal to bear on the subject. Where, for example, our great ornithologists of the early part of the century, such as Wilson and Audubon, had classed all our numerous hawks in a genus falco, later students split the group up into numerous genera—just how many it is impossible to say, as no two authorities agreed on that point. Wilson, could he have come back a generation after his death, would have found himself quite at a loss to converse with his successors about the birds he knew and loved so well, using their technical names—though the birds themselves had not changed.

Notwithstanding all the differences of opinion about matters of detail, however, there was, nevertheless, substantial agreement about the broader outlines of classifications, and it might fairly enough have been hoped that some day, when longer study had led to finer discrimination, the mysteries of all the types of creation would be fathomed. But then, while this hope still seemed far enough from realization, Charles Darwin came forward with his revolutionizing doctrine—and the whole time-honored myth of "types" of creation vanished in thin air. It became clear that the zoologists had been attempting a task utterly Sisyphean. They had sought to establish "natural groups" where groups do not exist in nature. They were eagerly peering after an ideal that had no existence outside their imagination. Their barriers of words could not be made to conform to barriers of nature, because in nature there are no barriers.

What, then, was to be done? Should the whole

fabric of classification be abandoned? Clearly not, since there can be no science without classification of facts about labelled groupings, however arbitrary. Classifications then must be retained, perfected; only in future it must be remembered that any classification must be more or less arbitrary, and in a sense false; that it is at best only a verbal convenience, not the embodiment of a final ideal. If, for example, we consider the very "natural" group of birds commonly called hawks, we are quite justified in dividing this group into several genera or minor groups, each composed of several species more like one another than like the members of other groups of species—that is, of other genera. But in so doing we must remember that if we could trace the ancestry of our various species of hawks we should find that in the remote past the differences that now separate the groups had been less and less marked, and originally quite non-existent, all the various species having sprung from a common ancestor. The genera of to-day are cousin-groups, let us say; but the parents of the existing species were of one brood, brothers and sisters. And what applies to the minor groups called genera applies also, going farther into the past, to all larger groups as well, so that in the last analysis, all existing creatures being really the evolved and modified descendants of one primordial type, it may be said that all animate creation is but a single kind. In this broadened view the details of classification ceased to have the importance once ascribed to them, and the quibblings of the classifiers seem amusing rather than serious.

Yet the changed point of view left the subject by no

means barren of interest. For if the multitudinous creatures of the living world are but diversified twiglets of a great tree of ascent, spread by branching from a common root, at least it is worth knowing what larger branches each group of twiglets—representing a genus, let us say—has sprung from. In particular, since the topmost twig of the tree is represented by man himself and his nearest relatives, is it of human interest to inquire just what branches and main stems will be come upon in tracing back the lineage of this particular offshoot. This attempt had, perhaps, no vast, vital importance in the utilitarian sense in which these terms are oftenest used, but at least it had human interest. Important or otherwise, it was the task that lay open to zoology, and apparently its only task, so soon as the Darwinian hypothesis had made good its status.

The man who first took this task in hand, and who has most persistently and wisely followed it, and hence the man who became the recognized leader in the field of the new zoology, was, as I have already intimated, Professor Haeckel. His hypothetical tree of man's lineage, tracing the ancestry of the human family back to the earliest geological times and the lowest orders of beings, has been familiar now for just a third of a century. It was at first confessedly only a tentative genealogy, with many weak limbs and untraced branches. It was perfected from time to time, as new data came to hand, through studies of paleontology, of embryology, and of comparative anatomy. It will be of interest, then, to inquire just what is its status today and to examine briefly Professor Haeckel's own most recent pronouncement regarding it.

Perhaps it is not worth our while here to go too far down towards the root of the genealogical tree to begin our inquiry. So long as it is admitted that the remote ancestry is grounded in the lowest forms of organisms, it perhaps does not greatly matter to the average reader that there are dark places in the lineage during the period when our ancestor had not yet developed a spinal column—when, in other words, he had not attained the dignity of the lowest fish. Neither, perhaps, need we mourn greatly that the exact branch by which our reptilian or amphibian non-mammalian ancestor became the first and most primitive of mammals is still hidden in unexplored recesses of early strata. The most patrician monarch of to-day would not be greatly disturbed as to just who were his ancestors of the days of the cave-dweller. It is when we come a little nearer home that the question begins to take on its seemingly personal significance. Questions of grandparents and great-grandparents concern the patrician very closely. And so all along, the question that has interested the average casual investigator of the Darwinian theory has been the question as to man's immediate ancestor—the parents and grandparents of our race, so to speak. Hence the linking of the word "monkey" with the phrase "Darwinian theory" in the popular mind; and hence, also, the interpretation of the phrase "missing link" in relation to man's ancestry, as applying only to our ancestor and not to any other of the gaps in the genealogical chain.

What, then, is the present status of Haeckel's genealogical tree regarding man's most direct ancestor? From what non-human parent did the human race

HAECKEL'S LABORATORY

directly spring? That is a question that has proved itself of lasting, vital human interest. It is a question that long was answered only with an hypothesis, but which Professor Haeckel to-day professes to be able to answer with a decisive and affirmative citation not of theories but of facts. In a word, it is claimed that man's immediate ancestor is now actually upon record, that the much-heralded "missing link" is missing no longer.

The principal single document, so to speak, on which this claim is based consists of the now famous skull and thigh-bone which the Dutch surgeon, Dr. Eugene Dubois, discovered in the year 1891 in the tertiary strata of the island of Java. Tertiary strata, it should be explained, had never hitherto yielded any fossils bordering on the human type, but this now famous skeleton was unmistakably akin to the human. The thigh in particular, taken by itself, would have been pronounced by any competent anatomist to be of human origin. Unquestionably the individual who bore it had been accustomed to take an erect attitude in walking. And yet the skull was far inferior in size and shape to that of any existing tribe of man—was, indeed, rather of a simian type, though, on the other hand, of about twice the capacity of any existing ape. In a word, it seemed clear that the creature whose part skeleton had been found by Dr. Dubois was of a type intermediate between the lowest existing man and the highest existing man-apes. It was, in short, the actual prototype of that hypothetical creature which Haeckel, in his genealogical tree, had christened *pithecanthropus*, the ape-man. As such it was christened *Pithecanthropus erectus*, the erect ape-man.

Now the discovery of this remarkable form did not make Professor Haeckel any more certain that some such form had existed than he was thirty years before when he christened a hypothetical subject with the title now taken by a tangible claimant. But, after all, there is something very taking about a prophecy fulfilled, and so the appearance of *Pithecanthropus erectus* created no small sensation in the zoological world. He was hailed by Haeckel and his followers as the veritable "missing link," and as such gained immediate notoriety. But, on the other hand, a reactionary party at once attacked him with the most bitter animadversions, denouncing him as no true ancestor of man with a bitterness that is hard to understand, considering that the origin of man from *some* lower form has long ceased to be matter of controversy. "*Pithecanthropus* is at least half an ape," they cried, with the clear implication of "anything but an ape for an ancestor!"

I confess I have always found it hard to understand just why this peculiar aversion should always be held against the unoffending ape tribe. Why it would not be quite as satisfactory to find one's ancestor in an ape as in the alternative lines of, for example, the cow, or the hippopotamus, or the whale, or the dog has always been a mystery. Yet the fact of this prejudice holds. Probably we dislike the ape because of the very patency of his human affinities. The poor relation is objectionable not so much because he is poor as because he is a relation. So, perhaps, it is not the apeness, so to speak, of the ape that is objectionable, but rather the humanness. In any event, the aversion has been matter of common notoriety ever since the Darwinian theory

became fully accepted; it showed itself now with renewed force against poor *pithecanthropus*. A half-score of objections were launched against him. It is needless to rehearse them now, since they were all met valiantly, and the final verdict saw the new-comer triumphantly ensconced in man's ancestral halls as the oldest sojourner there who has any title to be spoken of as "human." He is only half human, to be sure—a veritable ape-man, as his name implies—but exactly therein lies his altogether unique distinction. He is the embodiment of that "missing link" whose non-appearance had hitherto given so much comfort to the sceptical.

Perhaps some crumbs of comfort may be found by the reactionists in the fact that it is not held by Professor Haeckel, or by any other competent authority, that the link which *pithecanthropus* supplies welds man directly with any existing man-ape—with gorilla, chimpanzee, or orang. It is held that these highest existing apes are side branches, so to say, of the ancestral tree, who developed, in their several ways, contemporaneously with our direct ancestors, but are not themselves directly of the royal line. The existing ape that has clung closest to the direct ancestral type of our own race, it appears, is the gibbon—a creature far less objectionable in that rôle because of the very paucity of his human characteristics, as revealed to the casual observer. Gibbon-like fossil apes are known, in strata representing a time some millions of years antecedent to the epoch of *pithecanthropus* even, which are held to be directly of the royal line through which *pithecanthropus*, and the hypothetical *Homo stupidus*,

and the known *Homo neanderthalensis*, and, lastly, proud *Homo sapiens* himself have descended. Thus Professor Haeckel is able to make the affirmation, as he did recently before the International Zoological Congress in Cambridge, that man's line of descent is now clearly traced, from a stage back in the Eocene time when our ancestor was not yet more than half arrived to the ape's estate, down to the time of true human development. "There no longer exists," he says, "a 'missing link.' The phyletic continuity of the primate stem, from the oldest lemurs down to man himself, is an historical fact."

It should, perhaps, be added that the force of this rather startling conclusion rests by no means exclusively upon the finding of *pithecanthropus* and the other fossils, nor indeed upon any paleontological evidence whatever. These, of course, furnish data of a very tangible and convincing kind; but the evidence in its totality includes also a host of data from the realms of embryology and comparative anatomy—data which, as already suggested, enabled Professor Haeckel to predicate the existence of *pithecanthropus* long in advance of his actual discovery. Whether the more remote gaps in the chain of man's ancestry will be bridged in a manner similarly in accord with Professor Haeckel's predications, it remains for future discoveries of zoologist and paleontologist to determine. In any event, the recent findings have added an increment of glory to that philosophical zoology of which Professor Haeckel is the greatest living exponent.

This tracing of genealogies is doubtless the most spectacular feature of the new zoology, yet it must be

clear that the establishment of lines of evolution is at best merely a preparation for the all-important question, Why have these creatures, man included, evolved at all? That question goes to the heart of the new zoological philosophy. A partial answer was, of course, given by Darwin in his great doctrine of natural selection. But this doctrine, while explaining the preservation of favorable variations, made no attempt to account for the variations themselves. Professor Haeckel's contribution to the subject consisted in the revival of the doctrine of Lamarck, that individual variations, in response to environmental influences, are transmitted to the offspring, and thus furnish the material upon which, applying Darwin's principle, evolution may proceed. This Lamarck-Haeckel doctrine was under a cloud for a recent decade, during the brief passing of the Weismannian myth, but it has now emerged, and stands as the one recognized factor in the origin of those variations whose cumulative preservation through natural selection has resulted in the evolution of organic forms.

But may there not be other factors, as yet unrecognized, that supplement the Lamarckian and Darwinian principles in bringing about this marvellous evolution of beings? That, it would seem, is the most vital question that the philosophical zoology of our generation must hand on to the twentieth century. For today not even Professor Haeckel himself can give it answer.

VII

SOME MEDICAL LABORATORIES AND MEDICAL PROBLEMS

THE PASTEUR INSTITUTE

THE national egotism that characterizes the French mind is not without its compensations. It leads, for example, to the tangible recognition of the merits of the great men of the nation and to the promulgation of their names in many public ways. Thus it would be hard to mention a truly distinguished Frenchman of the older generations whose name has not been given to a street in Paris. Of the men of science thus honored, one recalls off-hand the names of Buffon, Cuvier, Geoffroy Saint-Hilaire, Pinel, Esquirol, Lamarck, Laplace, Lavoisier, Arago, Claude Bernard, Broca—indeed, one could readily extend the list to tiresome dimensions. Moreover, it is a list that is periodically increased by the addition of new names, as occasion offers, for the Parisian authorities never hesitate to rechristen a street or a portion of a street, regardless of former associations.

One of the most recent additions to this roll of fame is the name of Pasteur. The boulevard that bears that famous name is situated in a somewhat out-of-the-way corner of the city, though to reach it one has but to traverse the relatively short course of the Avenue de

Breteuil from so central a position as the tomb of Napoleon. The Boulevard Pasteur itself is a not long but very spacious thoroughfare, which will some day be very beautiful, when the character of its environing buildings has somewhat changed and its quadruple rows of trees have had time for development. At present its chief distinction, in the eyes of most observers, would probably be found in the fact that it is the location of the famous *fête forain* at one of the annually recurring stages of the endless itinerary of that noted function. During the period of this distinction, which falls in the month of May, the boulevard becomes transformed into a veritable Coney Island of merry-go-rounds, shooting-galleries, ginger-bread booths, and clap-trap side-shows, to the endless delight of throngs of pleasure-seekers. There is no sight in all Paris worthier inspection for the foreigner than the Boulevard Pasteur offers at this season, for one gains a deep insight into the psychology of a people through observation of the infantile delight with which the adult population here throws itself into the spirit of amusements which with other nations are for the most part reserved for school-children. Only a race either in childhood or senescence, it would seem, could thus give itself over with undisguised delight to the enchantments of wooden horses, cattle, cats, and pigs; to the catching of wooden fish with hooks; to the shooting at targets that one could almost touch with the gun-muzzle, and to the grave observation of side-show performances that would excite the risibilities of the most unsophisticated audience that could be found in the Mississippi Valley.

As we move among this light-hearted and light-headed throng we shall scarcely escape a feeling of good-humored contempt for what seems an inferior race. It will be wholesome, therefore, for us to turn aside from the boulevard into the Rue Dotot, which leads from it near its centre, and walk a few hundred yards away from the pleasure-seekers, where an evidence of a quite different and a no less characteristic phase of the national psychology will be before us. For here, within easy sound of the jangling discords of the organs that keep time for the march of the *cheveaux de bois*, rises up a building that is in a sense the monument of a man who was brother in blood and in sentiment to the revellers we have just left in the boulevard, yet whose career stamped him as one of the greatest men of genius of any race or any time. That man was Louis Pasteur. The building before us is the famous institute that bears his name.

In itself this building is a simple and unimposing structure, yet of pleasing contour. It is as well placed as the surroundings permit, on a grassed terrace, a little back from the street, where a high iron fence guards it and gives it a degree of seclusion. There are other buildings visible in the rear, which, as one learns on entering, are laboratories and the like, where the rabbits and guinea-pigs and dogs that are so essential to the work of the laboratory are kept. On the terrace in front is a bronze statue of a boy struggling with a rabid dog—a reminder of the particular labor of the master-worker which led directly to the foundation of the institution. It will be remembered that it was primarily to give Pasteur a wider opportunity to apply

ENTRANCE TO THE TOMB OF PASTEUR

his newly discovered treatment for the prevention of rabies that the subscription was undertaken which led finally to the erection of the buildings before us and brought the Pasteur Institute in its present form into being. Of the other aims and objects of the institution I shall speak more at length in a moment.

I have just said that the building before us is in effect the monument of the great savant. This is true in a somewhat more literal sense than might be supposed, for the body of Pasteur rests in a crypt at its base. The personal labors of the great discoverer were practically ended at the time when the institute was opened in 1888, on which occasion, as will be remembered, the scientific representatives of all nations gathered in Paris to do honor to the greatest Frenchman of his generation. He was spared to the world, however, for seven years more, during which time he fully organized the work of the institution along the lines it has since followed, and was, of course, the animating spirit of all the labors undertaken there by his devoted students and assistants. He is the animating spirit of the institution still, and it is fitting that his body should rest in the worthy mausoleum within the walls of that building whose erection was the tangible culmination of his life labors. The sarcophagus is a shrine within this temple of science which will serve to stimulate generations of workers here to walk worthily in the footsteps of the great founder of the institution. For he must be an unimaginative person indeed who, passing beneath that arch bearing the simple inscription "Ici Repose Pasteur," could descend into the simple but impressive mausoleum and stand

beside the massive granite sarcophagus without feeling the same kind of mental uplift which comes from contact with a great and noble personality. The pretentious tomb of Galileo in the nave of Santa Croce at Florence, and the crowded resting-place of Newton and Darwin in Westminster Abbey, have no such impressiveness as this solitary vault where rests the body of Pasteur, isolated in death as the mightier spirits must always be in life.

Aims and Objects of the Pasteur Institute

If one chances to come to the institute in the later hours of the morning he will perhaps be surprised to find a motley company of men, women, and children, apparently of many nationalities and from varied walks of life, gathered about one of the entrances or sauntering near by. These are the most direct beneficiaries of the institution, the unfortunate victims of the bites of rabid dogs, who have come here to take the treatment which alone can give them immunity from the terrible consequences of that mishap. Rabies, or hydrophobia as it is more commonly termed with us, is well known to be an absolutely fatal malady, there being no case on record of recovery from the disease once fully established. Even the treatment which Pasteur developed and which is here carried out cannot avail to save the victim in whom the active symptoms of the malady are actually present. But, fortunately, the disease is peculiarly slow in its onset, sometimes not manifesting itself for weeks or months after the inoculation; and this delay, which formerly was to the patient a period of fearful doubt and anxiety,

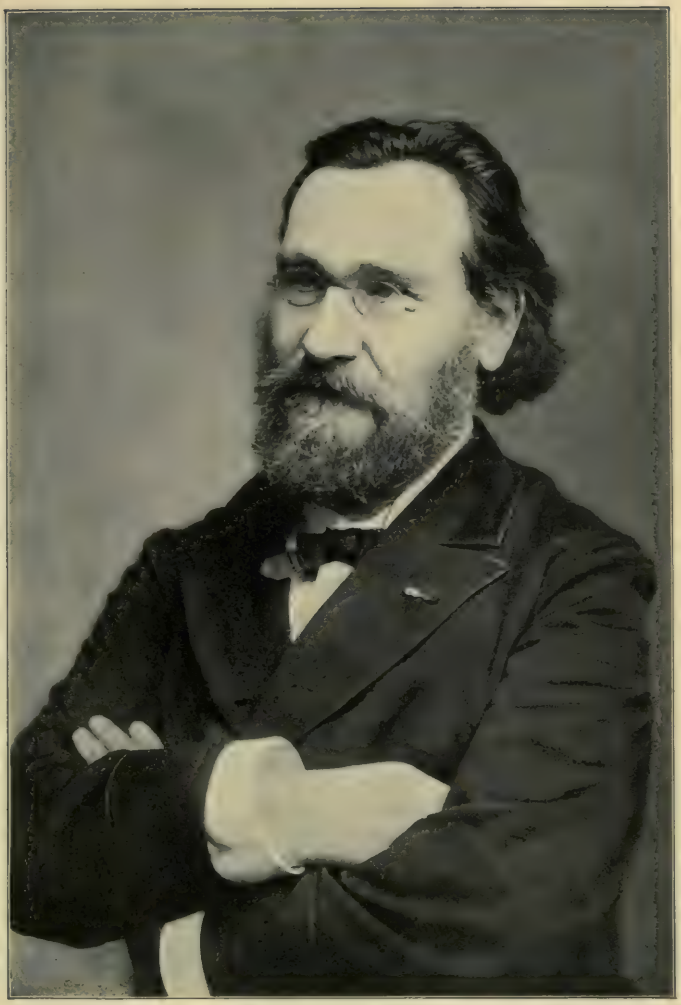
now suffices, happily, for the application of the protective inoculations which enable the person otherwise doomed to resist the poison and go unscathed. Thus it is that the persons who gather here each day to the number of fifty, or even one hundred, have the appearance of and the feelings of average health, though a large proportion of them bear in their systems, on arrival, the germs of a disease that would bring them speedily to a terrible end were it not that the genius of Pasteur had found a way to give them immunity.

The number of persons who have been given the anti-rabic treatment here is more than twenty-five thousand. To have given safety to such an army of unfortunates is, indeed, enough merit for any single institution; but it must not be supposed that this record is by any manner of means the full measure of the benefits which the Institut Pasteur has conferred upon humanity. In point of fact, the preparation and use of the anti-rabic serum is only one of many aims of the institution, whose full scope is as wide as the entire domain of contagious diseases. Pasteur's personal discoveries had demonstrated the relation of certain lower organisms, notably the bacteria, to the contagious diseases, and had shown the possibility of giving immunity from certain of these diseases through the use of cultures of the noxious bacteria themselves. He believed that these methods could be extended and developed until all the contagious diseases, which hitherto have accounted for so startling a proportion of all deaths, were brought within the control of medical science. His deepest thought in founding the institute was to supply a tangible seat of operations

for this attempted conquest, where the brilliant assistants he had gathered about him, and their successors in turn, might take a share in this great struggle, unhampered by the material drawbacks which so often confront the would-be worker in science.

He desired also that the institution should be a centre of education along the lines of its work, adding thus an indirect influence to the score of its direct achievements. In both these regards the institution has been and continues to be worthy of its founder. The Pasteur Institute is in effect a school of bacteriology, where each of the professors is at once a teacher and a brilliant investigator. The chief courses of instruction consist of two series each year of lectures and laboratory demonstrations on topics within the field of bacteriology. These courses, at which all the regular staff of the institution assist more or less, are open to physicians and other competent students regardless of nationality, and they suffice to inculcate the principles of bacteriology to a large band of seekers each year.

But more important, perhaps, than this form of educational influence is the impetus given by the institute to the researches of a small, select band of investigators who have taken up bacteriology for a life work, and who come here to perfect themselves in the final niceties of the technique of a most difficult profession. Thus such men as Calmette, the discoverer of the serum treatment of serpent-poisoning, and Yersin, famous for his researches in the prevention and cure of cholera by inoculation, are "graduates" of the Pasteur Institute. Indeed, almost all the chief laborers in this field in the world to-day, including the directors of



ELIAS METCHNIKOFF

practically all the daughter institutes bearing the same name that are now scattered all over the world, have had at least a share of their training in the mother institute here in Paris.

Of the work of the men who form the regular staff of the Pasteur Institute only a few words need be said here. Doctors Roux, Grancher, Metchnikoff, and Chamberland all had the privilege of sharing Pasteur's labors during the later years of the master's life, and each of them is a worthy follower of the beloved leader and at the same time a brilliant original investigator.[1] Roux is known everywhere in connection with the serum treatment of diphtheria, which he was so largely instrumental in developing. Grancher directs the anti-rabic department and allied fields. Metchnikoff, a Russian by birth and Parisian by adoption, is famous as the author of the theory that the white blood-corpuscles of the blood are the efficient agents in combating bacteria. Chamberland directs the field of practical bacteriology in its applications to hygiene, including the department in which protective serums are developed for the prevention of various diseases of domesticated animals, notably swine fever and anthrax. About one million sheep and half as many cattle are annually given immunity from anthrax by the serum here produced.

Of the patient and unremitting toil demanded of the investigator in this realm of the infinitely little; of the skill in manipulation, the fertility of resource, the scrupulous exactness of experiment that are absolutely prerequisite to success; of the dangers that attend investigations which deal with noxious germs, every one

who knows anything of the subject has some conception, but those alone can have full comprehension who have themselves attempted to follow the devious and delicate pathways of bacteriology. But the goals to which these pathways lead have a tangibility that give them a vital interest for all the world. The hopes and expectations of bacteriology halt at nothing short of the ultimate extirpation of contagious diseases. The way to that goal is long and hard, yet in time it will be made passable. And in our generation there is no company of men who are doing more towards that end than the staff of that most famous of bacteriological laboratories the Pasteur Institute.

THE VIRCHOW INSTITUTE OF PATHOLOGY

Even were the contagious diseases well in hand, there would still remain a sufficient coterie of maladies whose origin is not due to the influence of living germs. There are, for example, many diseases of the digestive, nutritive, and excretory systems, of the heart and arteries, of the brain and nerves, and various less clearly localized abnormal conditions, that owe their origin to inherent defects of the organism, or to various indiscretions of food or drink, to unhygienic surroundings, to material injuries, or to other forms of environmental stress quite dissociated from the action of bacteria. It is true that one would need to use extreme care nowadays in defining more exactly the diseases that thus lie without the field of the bacteriologist, as that prying individual seems prone to claim almost everything within sight, and to justify his claim with the microscope; but after that instrument has done its

best or worst, there will still remain a fair contingent of maladies that cannot fairly be brought within the domain of the ever-present "germ." On the other hand, all germ diseases have of course their particular effects upon the system, bringing their results within the scope of the pathologist. Thus while the bacteriologist has no concern directly with any disease that is not of bacterial origin, the pathologist has a direct interest in every form of disease whatever; in other words, bacteriology, properly considered, is only a special department of pathology, just as pathology itself is only a special department of general medicine.

Whichever way one turns in science, subjects are always found thus dovetailing into one another and refusing to be sharply outlined. Nevertheless, here as elsewhere, there are theoretical bounds that suffice for purposes of definition, if not very rigidly lived up to in practice; and we are justified in thinking of the pathologist (perhaps I should say the pathological anatomist) as the investigator of disease who is directly concerned with effects rather than with causes, who aims directly at the diseased tissue itself and reasons only secondarily to the causes. His problem is: given a certain disease (if I may be permitted this personified form of expression), to find what tissues of the body are changed by it from the normal and in what manner changed.

It requires but a moment's reflection to make it clear that a certain crude insight into the solution of this problem, as regards all common diseases, must have been the common knowledge of medical men since the earliest times. Thus not even medical knowledge

was needed to demonstrate that the tissues of an inflamed part become red and swollen; and numerous other changes of diseased tissues are almost equally patent. But this species of knowledge, based on microscopic inspection, was very vague and untrustworthy, and it was only after the advent of the perfected microscope, some three-quarters of a century ago, that pathological anatomy began to have any proper claim to scientific rank. Indeed, it was not until about the year 1865 that the real clew was discovered which gave the same impetus to pathology that the demonstration of the germ theory of disease gave at about the same time to etiology, or the study of causes of disease. This clew consisted of the final demonstration that all organic action is in the last resort a question of cellular activities, and, specifically, that all abnormal changes in any tissues of the body, due to whatever disease, can consist of nothing more than the destruction, or the proliferation, or the alteration of the cells that compose that tissue.

That seems a simple enough proposition nowadays, but it was at once revolutionary and inspiring in the day of its original enunciation some forty years ago. The man who had made the discovery was a young German physician, professor in the University of Freiburg, by name Rudolph Virchow. The discovery made him famous, and from that day to this the name of Virchow has held somewhat the same position in the world of pathology that the name of Pasteur occupied in the realm of bacteriology. Virchow was called presently to a professorship in the University of Berlin. In connection with this chair he established his fa-

mous Institute of Pathology, which has been the Mecca of all students of pathology ever since. He did a host of other notable things as well, among others, entering the field of politics, and becoming a recognized leader there no less than in science. Indeed, it seemed during the later decades of his life as if one encountered Virchow in whatever direction one turned in Berlin, and one feels that it was not without reason that his compatriots spoke of him as "the man who knows everything." To the end he retained all the alertness of intellect and the energy of body that had made him what he was. One found him at an early hour in the morning attending to the routine of his hospital duties, his lectures, and clinical demonstrations. These finished, he rushed off, perhaps to his parliamentary duties; thence to a meeting of the Academy of Sciences, or to preside at the Academy of Medicine or at some other scientific gathering. And in intervals of these diversified pursuits he was besieged ever by a host of private callers, who sought his opinion, his advice, his influence in some matter of practical politics, of statecraft, or of science, or who, perhaps, had merely come the length of the continent that they might grasp the hand of the "father of pathology."

In whatever capacity one sought him out, provided the seeking were not too presumptuous, one was sure to find the great savant approachable, courteous, even cordial. A man of multifarious affairs, he impressed one as having abundance of time for them all, and to spare. There is a leisureliness about the seeming habit of existence on the Continent that does not pertain in America, and one felt the flavor of it quite as

much in the presence of this great worker as among those people who from our stand-point seem never really to work at all. This is to a certain extent explained if one visited Virchow in his home, and found to his astonishment that the world-renowned physician, statesman, pathologist, anthropologist was domiciled in a little apartment of the most modest equipment, up two flights, in a house of most unpretentious character. Everything was entirely respectable, altogether comfortable, to be sure; but it was a grade of living which a man of corresponding position in America could not hold to without finding himself quite out of step with his confrères and the subject of endless comment. But in this city of universal apartment-house occupancy and relatively low average of display in living it is quite otherwise. Virchow lived on the same plane, generally speaking, with the other scientists of Europe; it is only from the American stand-point that there is any seeming disparity between his fame and his material station in life; nor do I claim this as a merit of the American stand-point.

Be that as it may, however, our present concern lies not with these matters, but with Virchow the pathologist and teacher. To see the great scientist at his best in this rôle, it was necessary to visit the Institute of Pathology on a Thursday morning at the hour of nine. On the morning of our visit we found the students already assembled and gathered in clusters all about the room, examining specimens of morbid anatomy, under guidance of various laboratory assistants. This was to give them a general familiarity with the appearances of the disease-products that would be

described to them in the ensuing lecture. But what is most striking about the room was the very unique method of arrangement of the desk or table on which the specimens rested. It was virtually a long-drawn-out series of desks winding back and forth throughout the entire room, but all united into one, so that a specimen passed along the table from end to end will make a zigzag tour of the room, passing finally before each person in the entire audience. To facilitate such transit, there was a little iron railway all along the centre of the table, with miniature turn-tables at the corners, along which microscopes, with adjusted specimens for examination, might be conveyed without danger of maladjustment or injury. This may seem a small detail, but it is really an important auxiliary in the teaching by demonstration with specimens for which this room was peculiarly intended. The ordinary lectures of Professor Virchow were held in a neighboring amphitheatre of conventional type.

Of a sudden there was a hush in the hum of voices, as a little, thin, frail-seeming man entered and stepped briskly to the front of the room and upon the low platform before the blackboard in the corner. A moment's pause for the students to take their places, and the lecturer, who of course was Virchow himself, began, in a clear, conversational voice, to discourse on the topic of the day, which chanced to be the formation of clots in blood-vessels. There was no particular attempt at oratory; rather the lecturer proceeded as if talking man to man, with no thought but to make his meaning perfectly clear. He began at once putting specimens in circulation, as supplied on his demand by

his assistants from a rather grewsome-looking collection before him. Now he paused to chaff the assistant who was making the labels, poking good-humored jokes at his awkwardness, but with no trace of sting. Again he became animated, his voice raised a little, his speech more vehement, as he advanced his own views on some contested theory or refuted the objections that some opponent had urged against him, always, however, with a smile lurking about his eyes or openly showing on his lips.

Constantly the lecturer turned to the blackboard to illustrate with colored crayons such points of his discourse as the actual specimens in circulation might leave obscure. Everything must be made plain to every hearer or he would not be satisfied. One can but contrast such teaching as this with the lectures of the average German professor, who seems not to concern himself in the least as to whether anything is understood by any one. But Virchow had the spirit of the true teacher. He had the air of loving his task, old story as it was to him. Most of his auditors were mere students, yet he appealed to them as earnestly as if they were associates and equals. He seemed to try to put himself on their level—to make his thought near to them. Physically he was near to them as he talked, the platform on which he stood being but a few inches in height, and such physical nearness conduces to a familiarity of discourse that is best fitted for placing lecturer and hearers *en rapport*. All in all, appealing as it does almost equally to ear and eye, it is a type of what a lecturer should be. Not a student there but went away with an added fund of information,

PROFESSOR RUDOLF VIRCHOW IN HIS LABORATORY

which is far more than can be said of most of the lectures in a German university.

Needless to say, there are other departments to the Institute of Pathology. There are collections of beautifully preserved specimens for examination; rooms for practical experimentation in all phases of the subject, the chemical side included; but these are not very different from the similar departments of similar institutions everywhere. What was unique and characteristic about this institution was the personality of the director. Now he is gone, but his influence will not soon be forgotten. The pupils of a great teacher are sure to carry forward the work somewhat in the spirit of the master for at least a generation.

THE BERLIN INSTITUTE OF HYGIENE

I purposely refrain from entering into any details as to the character of the technical work done at the Virchow Institute, because the subject of pathology, despite its directly practical bearings, is in itself necessarily somewhat removed from the knowledge of the general reader. One cannot well understand the details of changes in tissues under abnormal conditions unless one first understands the normal conditions of the tissues themselves, and such knowledge is reserved for the special students of anatomy. For the non-professional observer the interest of the Virchow Institute must lie in its general scope rather than in the details of the subjects there brought under investigation, which latter have, indeed, of necessity, a somewhat grewsome character despite the beneficent results that spring from them. It is quite otherwise,

however, with the work of the allied institution of which I now come to speak. The Institute of Hygiene deals with topics not very remote from those studied in the Virchow Institute, part of its work, indeed, falling clearly within the scope of pathology; but it differs in being clearly comprehensible to the general public and of immediate and tangible interest from the most strictly utilitarian stand-point, hygiene being, in effect, the tangible link between the more abstract medical sciences and the affairs of every-day life.

The Institute of Hygiene has also the interest that always attaches to association with a famous name, for it was here that Professor Koch made the greater part of those investigations which made his name the best known, next to that of Pasteur, of any in the field of bacteriology. In particular, the researches on the cholera germ, and those even more widely heralded researches that led to the discovery of the bacillus of tuberculosis, and the development of the remedy tuberculin, of which so much was at first expected, were made by Professor Koch in the laboratories of the antiquated building which was then and is still the seat of the Institute of Hygiene. More recently Professor Koch has severed his connection with the institution after presiding over it for many years, having now a semi-private laboratory just across from the Virchow Institute, in connection with the Charité Hospital; but one still thinks of the Institute of Hygiene as peculiarly the "Koch Institute" without injustice, so fully does its work follow the lines laid out for it by the great leader.

But however much the stamp of any individual per-

sonality may rest upon the institute, it is officially a department of the university, just as is the Virchow Institute. Like the latter, also, its local habitation is an antiquated building, strangely at variance, according to American ideas, with its reputation, though by no means noteworthy in this regard in the case of a German institution. It is situated in a part of the city distant from any other department of the university, and there is nothing about it exteriorly to distinguish it from other houses of the solid block in which it stands. Interiorly, it reminds one rather of a converted dwelling than a laboratory proper. Its rooms are well enough adapted to their purpose, but they give one the impression of a makeshift. The smallest American college would be ill-satisfied with such an equipment for any department of its work. Yet in these dingy quarters has been accomplished some of the best work in the new science of bacteriology that our century will have to boast.

The actual equipment of the bacteriological laboratory here is not, indeed, quite as meagre as it seems at first, there being numerous rooms, scattered here and there, which in the aggregate give opportunity for work to a large number of investigators, though no single room makes an impressive appearance. There is one room, however, large enough to give audience to a considerable class, and here lectures were given by Professor Koch and continue to be given by his successors to the special students of bacteriology who come from all over the world, as well as to the university students who take the course as a part of their regular medical curriculum. In regard to this feature

of its work, the Institute of Hygiene differs in no essential respect from the Pasteur Institute and other laboratories of bacteriology. The same general routine of work pertains: the patient cultivation of the minute organisms in various mediums, their careful staining by special processes, and their investigation under the microscope mark the work of the bacteriologist everywhere. Many details of the special methods of culture or treatment originated here with Professor Koch, but such matters are never kept secret in science, so one may see them practised quite as generally and as efficiently in other laboratories as in this one. Indeed, it may frankly be admitted that, aside from its historical associations with the pioneer work in bacteriology, which will always make it memorable, there is nothing about the bacteriological laboratory here to give it distinction over hundreds of similar ones elsewhere; while in point of technical equipment, as already noted, it is remarkable rather for what it lacks than for what it presents.

The department of bacteriology, however, is only one of several important features of the institute. One has but to ascend another flight of stairs to pass out of the sphere of the microbe and enter a department where attention is directed to quite another field. We have now come to what may be considered the laboratory of hygiene proper, since here the investigations have to do directly with the functionings of the human body in their relations to the every-day environment. Here again one is struck with the meagre equipment with which important results may be attained by patient and skilled investigators. In only one room does

one find a really elaborate piece of apparatus. This exceptional mechanism consists essentially of a cabinet large enough to give comfortable lodgment to a human subject—a cabinet with walls of peculiar structure, partly of glass, and connected by various pipes with sundry mysterious-seeming retorts. This single apparatus, however, is susceptible of being employed for the investigation of an almost endless variety of questions pertaining to the functionings of the human body considered as a working mechanism.

Thus, for example, a human subject to be experimented upon may remain for an indefinite period within this cabinet, occupied in various ways, taking physical exercise, reading, engaged in creative mental labor, or sleeping. Meantime, air is supplied for respiration in measured quantities, and of a precisely determined composition, as regards chemical impurities, moisture, and temperature. The air after passing through the chamber being again analyzed, the exact constituents added to it as waste products of the human machine in action under varying conditions are determined. It will readily be seen that by indefinitely varying the conditions of such experiments a great variety of data may be secured as to the exact physiological accompaniments of various bodily and mental activities. Such data are of manifest importance to the physiologist and pathologist on the one hand, while at the same time having a direct bearing on such eminently practical topics as the construction of shops, auditoriums, and dwellings in reference to light, heat, and ventilation. It remains only for practical architecture to take advantage of the unequivocal data thus placed at

its disposal—an opportunity of which practical architecture, in Germany as elsewhere on the Continent, has hitherto been very slow to avail itself.

The Museum of Hygiene

The practical lessons thus given in the laboratory are supplemented in an even more tangible manner, because in a way more accessible to the public, in another department of the institution which occupies a contiguous building, and is known as the Museum of Hygiene. This, unlike the other departments of the institute, is open to the general public on certain days of each week, and it offers a variety of exhibits of distinctly novel character and of high educational value. The general character of the exhibits may be inferred from the name, but perhaps the scope is even wider than might be expected. In a word, it may be said that scarcely anything having to do with practical hygiene has been overlooked. Thus one finds here numberless models of dwelling-houses, showing details of lighting, heating, and ventilation; models not merely of individual dwellings, but also of school-buildings, hospitals, asylums, and even prisons. Sometimes the models represent merely ideal buildings, but more generally they reproduce in miniature actual habitations. In the case of the public buildings, the model usually includes not merely the structures themselves but the surroundings—lawns, drives, trees, out-buildings—so that one can get a very good idea of the more important hospitals, asylums, and prisons of Germany by making a tour of the Museum of Hygiene. Regarding the details of structure, one can actually gain a fuller knowl-

edge in many cases than he could obtain by actual visits to the original institutions themselves.

The same thing is true of various other features of the subjects represented. Thus there is a very elaborate model here exhibited of the famous Berlin system of sewage-disposal. As is well known, the essential features of this system consist of the drainage of sewage into local reservoirs, from which it is forced by pumps, natural drainage not sufficing, to distant fields, where it is distributed through tile pipes laid in a network about a yard beneath the surface of the soil. The fields themselves, thus rendered fertile by the waste products of the city, are cultivated, and yield a rich harvest of vegetables and grains of every variety suitable to the climate. The visitor to this field sees only rich farms and market-gardens under ordinary process of cultivation. The system of pipes by which the land is fertilized is as fully hidden from his view as are, for example, the tributary sewage-pipes beneath the city pavements. The average visitor to Berlin knows nothing, of course, about one or the other, and goes away, as he came, ignorant of the important fact that Berlin has reached a better solution of the great sewage problem than has been attained by any other large city. Such, at least, is likely to be the case unless the sight-seer chance to pay a visit to the Museum of Hygiene, in which case a few minutes' inspection of the model there will make the matter entirely clear to him. It is to be regretted that the authorities of other large cities do not make special visits to Berlin for this purpose; though it should be added that some of them have done so, and that the Berlin system of "canalization"

has been adopted in various places in America. But many others might wisely follow their example, notably the Parisians, whose sewerage system, despite the boasted exhibition canal-sewer, is, like so many other things Parisian, of the most primitive character and a reproach to present-day civilization.

It may be added that there are plenty of things exhibited in this museum which the Germans themselves might study to advantage, for it must be understood that the other hygienic conditions pertaining to Berlin are by no means all on a par with the high modern standard of the sewerage system. In the matter of ventilation, for example, one may find admirable models in the museum, showing just how the dwelling and shop and school-room should make provision for a proper supply of pure air for their occupants. But if one goes out from the museum and searches in the actual dwelling or shop or school-room for the counterparts of these models, one will be sorely puzzled where to find them. The general impression which a casual inspection will leave in his mind is that the word ventilation must be as meaningless to the German mind as it is, for example, to the mind of a Frenchman or an Italian. This probably is not quite just, since the German has at least reached the stage of having museum models of ventilated houses, thus proving that the idea does exist, even though latent, in his mental equipment, whereas the other continental nationalities seem not to have reached even this incipient stage of progress. All over Europe the people fear a current of air as if veritable miasm must lurk in it. They seem quite oblivious to any systematic necessity

for replenishing the oxygen supply among large assemblies, as any one can testify who has, for example, visited their theatres or schools. And as to the private dwellings, after making them as nearly air-tight as practicable, they endeavor to preserve the *status quo* as regards air supply seemingly from season to season. They even seem to have passed beyond a mere negative regard for the subject of fresh air, inasmuch as they will bravely assure you that to sleep in a room with an open window will surely subject you to the penalty of inflamed eyes.

In a country like France, where the open fireplace is the usual means employed to modify the temperature (I will not say warm the room), the dwellings do of necessity get a certain amount of ventilation, particularly since the windows are not usually of the best construction. But the German, with his nearly air-tight double windows and his even more nearly sealed tile stove, spends the winter in an atmosphere suggestive of the descriptions that arctic travellers give us of the air in the hut of an Eskimo. It is clear, then, that the models in the Museum of Hygiene have thus far failed of the proselyting purpose for which they were presumably intended. How it has chanced that the inhabitants of the country maintain so high an average of robust health after this open defiance is a subject which the physiological department of the Institute of Hygiene might well investigate.

Even though the implied precepts of the Museum of Hygiene are so largely disregarded, however, it must be admitted that the existence of the museum is a hopeful sign. It is a valuable educational institution, and

if its salutary lessons are but slowly accepted by the people, they cannot be altogether without effect. At least the museum proves that there are leaders in science here who have got beyond the range of eighteenth-century thought in matters of practical living, and the sign is hopeful for the future, though its promise will perhaps not be fulfilled in our generation.

VII

SOME UNSOLVED SCIENTIFIC PROBLEMS

IN recent chapters we have witnessed a marvellous development in many branches of pure science. In viewing so wonderfully diversified a field, it has of course been impossible to dwell upon details, or even to glance at every minor discovery. At best one could but summarize the broad sweep of progress somewhat as a battle might be described by a distant eye-witness, telling of the general direction of action, of the movements of large masses, the names of leaders of brigades and divisions, but necessarily ignoring the lesser fluctuations of advance or recession and the individual gallantry of the rank and file. In particular, interest has centred upon the storming of the various special strongholds of ignorant or prejudiced opposition, which at last have been triumphantly occupied by the band of progress. In each case where such a stronghold has fallen, the victory has been achieved solely through the destructive agency of newly discovered or newly marshalled facts—the only weapons which the warrior of science seeks or cares for. Facts must be marshalled, of course, about the guidon of a hypothesis, but that guidon can lead on to victory only when the facts themselves support it. Once planted victoriously on the conquered ramparts the hypothesis becomes a theory—a generalization of science—marking a fresh

coign of vantage, which can never be successfully assailed unless by a new host of antagonistic facts. Such generalizations, with the events leading directly up to them, have chiefly occupied our attention.

But a moment's reflection makes it clear that the battle of science, thus considered, is ever shifting ground and never ended. Thus at any given period there are many unsettled skirmishes under way; many hypotheses are yet only struggling towards the stronghold of theory, perhaps never to attain it; in many directions the hosts of antagonistic facts seem so evenly matched that the hazard of war appears uncertain; or, again, so few facts are available that as yet no attack worthy the name is possible. Such unsettled controversies as these have, for the most part, been ignored in our survey of the field. But it would not be fair to conclude our story without adverting to them, at least in brief; for some of them have to do with the most comprehensive and important questions with which science deals, and the aggregate number of facts involved in these unfinished battles is often great, even though as yet the marshalling has not led to final victory for any faction. In some cases, doubtless, the right hypothesis is actually in the field, but its supremacy not yet conclusively proved—perhaps not to be proved for many years or decades to come. Some of the chief scientific results of the nineteenth century have been but the gaining of supremacy for hypotheses that were mere forlorn hopes, looked on with general contempt, if at all heeded, when the eighteenth century came to a close—witness the doctrines of the great age of the earth, of the immateriality of heat, of the un-

dulatory character of light, of chemical atomicity, of organic evolution. Contrariwise, the opposite ideas to all of these had seemingly a safe supremacy until the new facts drove them from the field. Who shall say, then, what forlorn hope of to-day's science may not be the conquering host of to-morrow? All that one dare attempt is to cite the pretensions of a few hypotheses that are struggling over the still contested ground.

SOLAR AND TELLURIC PROBLEMS

Our sun being only a minor atom of the stellar pebble, solar problems in general are of course stellar problems also. But there are certain special questions regarding which we are able to interrogate the sun because of his proximity, and which have, furthermore, a peculiar interest for the residents of our little globe because of our dependence upon this particular star. One of the most far-reaching of these is as to where the sun gets the heat that he gives off in such liberal quantities. We have already seen that Dr. Mayer, of conservation-of-energy fame, was the first to ask this question. As soon as the doctrine of the persistence and convertibility of energy was grasped, about the middle of the century, it became clear that this was one of the most puzzling of questions. It did not at all suffice to answer that the sun is a ball of fire, for computation showed that, at the present rate of heat-giving, if the sun were a solid mass of coal, he would be totally consumed in about five thousand years. As no such decrease in size as this implies had taken place within historic times, it was clear that some other explanation must be sought.

Dr. Mayer himself hit upon what seemed a tenable solution at the very outset. Starting from the observed fact that myriads of tiny meteorites are hurled into the earth's atmosphere daily, he argued that the sun must receive these visitants in really enormous quantities—sufficient, probably, to maintain his temperature at the observed limits. There was nothing at all unreasonable about this assumption, for the amount of energy in a swiftly moving body capable of being transformed into heat if the body be arrested is relatively enormous. Thus it is calculated that a pound of coal dropped into the sun from the mathematician's favorite starting-point, infinity, would produce some six thousand times the heat it could engender if merely burned at the sun's surface. In other words, if a little over two pounds of material from infinity were to fall into each square yard of the sun's surface each hour, his observed heat would be accounted for; whereas almost seven tons per square yard of stationary fuel would be required each hour to produce the same effect.

In view of the pelting which our little earth receives, it seemed not an excessive requisition upon the meteoric supply to suppose that the requisite amount of matter may fall into the sun, and for a time this explanation of his incandescence was pretty generally accepted. But soon astronomers began to make calculations as to the amount of matter which this assumption added to our solar system, particularly as it aggregated near the sun in the converging radii, and then it was clear that no such mass of matter could be there without interfering demonstrably with the observed course of the interior

planets. So another source of the sun's energy had to be sought. It was found forthwith by that other great German, Helmholtz, who pointed out that the falling matter through which heat may be generated might just as well be within the substance of the sun as without—in other words, that contraction of the sun's heated body is quite sufficient to account for a long-sustained heat-supply which the mere burning of any known substance could not approach. Moreover the amount of matter thus falling towards the sun's centre being enormous—namely, the total substance of the sun—a relatively small amount of contraction would be theoretically sufficient to keep the sun's furnace at par, so to speak.

At first sight this explanation seemed a little puzzling to many laymen and some experts, for it seemed to imply, as Lord Kelvin pointed out, that the sun contracts because it is getting cooler, and gains heat because it contracts. But this feat is not really as paradoxical as it seems, for it is not implied that there is any real gain of heat in the sun's mass as a whole, but quite the reverse. All that is sought is an explanation of a maintenance of heat-giving capacity relatively unchanged for a long, but not an interminable, period. Indeed, exactly here comes in the novel and startling feature of Helmholtz's calculation. According to Mayer's meteoric hypothesis, there were no data at hand for any estimate whatever as to the sun's permanency, since no one could surmise what might be the limits of the meteoric supply. But Helmholtz's estimate implied an incandescent body cooling—keeping up a somewhat equable temperature through contraction for a

time, but for a limited time only; destined ultimately to become liquid, solid; to cool below the temperature of incandescence—to die. Not only so, but it became possible to calculate the limits of time within which this culmination would probably occur. It was only necessary to calculate the total amount of heat which could be generated by the total mass of our solar system in falling together to the sun's centre from "infinity" to find the total heat-supply to be drawn upon. Assuming, then, that the present observed rate of heat-giving has been the average maintained in the past, a simple division gives the number of years for which the original supply is adequate. The supply will be exhausted, it will be observed, when the mass comes into stable equilibrium as a solid body, no longer subject to contraction, about the sun's centre—such a body, in short, as our earth is at present.

This calculation was made by Lord Kelvin, Professor Tait, and others, and the result was one of the most truly dynamitic surprises of the century. For it transpired that, according to mathematics, the entire limit of the sun's heat-giving life could not exceed something like twenty-five millions of years. The publication of that estimate, with the appearance of authority, brought a veritable storm about the heads of the physicists. The entire geological and biological worlds were up in arms in a trice. Two or three generations before, they hurled brickbats at any one who even hinted that the solar system might be more than six thousand years old; now they jeered in derision at the attempt to limit the life-bearing period of our globe to a paltry fifteen or twenty millions.

The controversy as to solar time thus raised proved one of the most curious and interesting scientific disputations of the century. The scene soon shifted from the sun to the earth; for a little reflection made it clear that the data regarding the sun alone were not sufficiently definite. Thus Dr. Croll contended that if the parent bodies of the sun had chanced to be "flying stars" before collision, a vastly greater supply of heat would have been engendered than if the matter merely fell together. Again, it could not be overlooked that a host of meteors are falling into the sun, and that this source of energy, though not in itself sufficient to account for all the heat in question, might be sufficient to vitiate utterly any exact calculations. Yet again, Professor Lockyer called attention to another source of variation, in the fact that the chemical combination of elements hitherto existing separately must produce large quantities of heat, it being even suggested that this source alone might possibly account for all the present output. On the whole, then, it became clear that the contraction theory of the sun's heat must itself await the demonstration of observed shrinkage of the solar disk, as viewed by future generations of observers, before taking rank as an incontestable theory, and that computations as to time based solely on this hypothesis must in the mean time be viewed askance.

But the time controversy having taken root, new methods were naturally found for testing it. The geologists sought to estimate the period of time that must have been required for the deposit of the sedimentary rocks now observed to make up the outer crust of the earth. The amount of sediment carried through the

mouth of a great river furnishes a clew to the rate of denudation of the area drained by that river. Thus the studies of Messrs. Humphreys and Abbot, made for a different purpose, show that the average level of the territory drained by the Mississippi is being reduced by about one foot in six thousand years. The sediment is, of course, being piled up out in the Gulf at a proportionate rate. If, then, this be assumed to be an average rate of denudation and deposit in the past, and if the total thickness of sedimentary deposits of past ages were known, a simple calculation would show the age of the earth's crust since the first continents were formed. But unfortunately these "ifs" stand mountain-high here, all the essential factors being indeterminate. Nevertheless, the geologists contended that they could easily make out a case proving that the constructive and destructive work still in evidence, to say nothing of anterior revolutions, could not have been accomplished in less than from twenty-five to fifty millions of years.

This computation would have carried little weight with the physicists had it not chanced that another computation of their own was soon made which had even more startling results. This computation, made by Lord Kelvin, was based on the rate of loss of heat by the earth. It thus resembled the previous solar estimate in method. But the result was very different, for the new estimate seemed to prove that a period of from one hundred to two hundred millions of years has elapsed since the final crust of the earth formed.

With this all controversy ceased, for the most grasp-

ing geologist or biologist would content himself with a fraction of that time. But the case for the geologist was to receive yet another prop from the studies of radio-activity, which seem to prove that the atom of matter has in store a tremendous supply of potential energy which may be drawn on in a way to vitiate utterly all the computations to which I have just referred. Thus a particle of radium is giving out heat incessantly in sufficient quantity to raise its own weight of water to the boiling-point in an hour. The demonstrated wide distribution of radio-active matter—making it at least an open question whether all matter does not possess this property in some degree—has led to the suggestion that the total heat of the sun may be due to radio-active matter in its substance. Obviously, then, all estimates of the sun's age based on the heat-supply must for the present be held quite in abeyance. What is more to the point, however, is the fact, which these varying estimates have made patent, that computations of the age of the earth based on any data at hand are little better than rough guesses. Long before the definite estimates were undertaken, geologists had proved that the earth is very, very old, and it can hardly be said that the attempted computations have added much of definiteness to that proposition. They have, indeed, proved that the period of time to be drawn upon is not infinite; but the nebular hypothesis, to say nothing of common-sense, carried us as far as that long ago.

If the computations in question have failed of their direct purpose, however, they have been by no means lacking in important collateral results. To mention

but one of these, Lord Kelvin was led by this controversy over the earth's age to make his famous computation in which he proved that the telluric structure, as a whole, must have at least the rigidity of steel in order to resist the moon's tidal pull as it does. Hopkins had, indeed, made a somewhat similar estimate as early as 1839, proving that the earth's crust must be at least eight hundred or a thousand miles in thickness; but geologists had utterly ignored this computation, and the idea of a thin crust on a fluid interior had continued to be the orthodox geological doctrine. Since Lord Kelvin's estimate was made, his claim that the final crust of the earth could not have formed until the mass was solid throughout, or at least until a honeycomb of solid matter had been bridged up from centre to circumference, has gained pretty general acceptance. It still remains an open question, however, as to what proportion the lacunæ of molten matter bear at the present day to the solidified portions, and therefore to what extent the earth will be subject to further shrinkage and attendant surface contortions. That some such lacunæ do exist is demonstrated daily by the phenomena of volcanoes. So, after all, the crust theory has been supplanted by a compromise theory rather than completely overthrown, and our knowledge of the condition of the telluric depths is still far from definite.

If so much uncertainty attends these fundamental questions as to the earth's past and present, it is not strange that open problems as to her future are still more numerous. We have seen how, according to Professor Darwin's computations, the moon threatens to come back to earth with destructive force some

day. Yet Professor Darwin himself urges that there are elements of fallibility in the data involved that rob the computation of all certainty. Much the same thing is true of perhaps all the estimates that have been made as to the earth's ultimate fate. Thus it has been suggested that, even should the sun's heat not forsake us, our day will become month-long, and then year-long; that all the water of the globe must ultimately filter into its depths, and all the air fly off into space, leaving our earth as dry and as devoid of atmosphere as the moon; and, finally, that ether-friction, if it exist, or, in default of that, meteoric friction, must ultimately bring the earth back to the sun. But in all these prognostications there are possible compensating factors that vitiate the estimates and leave the exact results in doubt. The last word of the cosmic science of our generation is a prophecy of evil—if annihilation be an evil. But it is left for the science of another generation to point out more clearly the exact terms in which the prophecy is most likely to be fulfilled.

PHYSICAL PROBLEMS

In regard to all these cosmic and telluric problems, it will be seen, there is always the same appeal to one central rule of action—the law of gravitation. When we turn from macrocosm to microcosm it would appear as if new forces of interaction were introduced in the powers of cohesion and of chemical action of molecules and atoms. But Lord Kelvin has argued that it is possible to form such a conception of the forms and space relations of the ultimate particles of matter that their mutual attractions may be explained by invoking that

same law of gravitation which holds the stars and planets in their course. What, then, is this all-compassing power of gravitation which occupies so central a position in the scheme of mechanical things?

The simple answer is that no man knows. The wisest physicist of to-day will assure you that he knows absolutely nothing of the why of gravitation—that he can no more explain why a stone tossed into the air falls back to earth than can the boy who tosses the stone. But while this statement puts in a nutshell the scientific status of explanations of gravitation, yet it is not in human nature that speculative scientists should refrain from the effort to explain it. Such efforts have been made; yet, on the whole, they are surprisingly few in number; indeed, there are but two that need claim our attention here, and one of these has hardly more than historical interest. One of these is the so-called ultramundane-corpuscle hypothesis of Le Sage; the other is based on the vortex theory of matter.

The theory of Le Sage assumes that the entire universe is filled with infinitely minute particles flying in right lines in every direction with inconceivable rapidity. Every mass of tangible matter in the universe is incessantly bombarded by these particles, but any two non-contiguous masses (whether separated by an infinitesimal space or by the limits of the universe) are mutually shielded by one another from a certain number of the particles, and thus impelled towards one another by the excess of bombardment on their opposite sides. What applies to two masses applies also, of course, to any number of masses—in short, to all the

matter in the universe. To make the hypothesis workable, so to say, it is necessary to assume that the "ultramundane" particles are possessed of absolute elasticity, so that they rebound from one another on collision without loss of speed. It is also necessary to assume that all tangible matter has to an almost unthinkable degree a sievelike texture, so that the vast proportion of the coercive particles pass entirely through the body of any mass they encounter—a star or world, for example—without really touching any part of its actual substance. This assumption is necessary because gravitation takes no account of mere corporeal bulk, but only of mass or ultimate solidarity. Thus a very bulky object may be so closely meshed that it retards relatively few of the corpuscles, and hence gravitates with relative feebleness—or, to adopt a more familiar mode of expression, is light in weight.

This is certainly heaping hypotheses together in a reckless way, and it is perhaps not surprising that Le Sage's conception did not at first arouse any very great amount of interest. It was put forward about a century ago, but for two or three generations remained practically unnoticed. The philosophers of the first half of our century seem to have despaired of explaining gravitation, though Faraday long experimented in the hope of establishing a relation between gravitation and electricity or magnetism. But not long after the middle of the century, when a new science of dynamics was claiming paramount importance, and physicists were striving to express all tangible phenomena in terms of matter in motion, the theory of Le Sage was revived and given a large measure of attention. It

seemed to have at least the merit of explaining the facts without conflicting with any known mechanical law, which was more than could be said of any other guess at the question that had ever been made.

More recently, however, another explanation has been found which also meets this condition. It is a conception based, like most other physical speculations of the last generation, upon the hypothesis of the vortex atom, and was suggested, no doubt, by those speculations which consider electricity and magnetism to be conditions of strain or twist in the substance of the universal ether. In a word, it supposes that gravitation also is a form of strain in this ether—a strain that may be likened to a suction which the vortex atom is supposed to exert on the ether in which it lies. According to this view, gravitation is not a push from without, but a pull from within; not due to exterior influences, but an inherent and indissoluble property of matter itself. The conception has the further merit of correlating gravitation with electricity, magnetism, and light, as a condition of that strange ethereal ocean of which modern physics takes so much account. But here, again, clearly, we are but heaping hypothesis upon hypothesis, as before. Still, an hypothesis that violates no known law and has the warrant of philosophical probability is always worthy of a hearing. But we must not forget that it is hypothesis only, not conclusive theory.

The same caution applies, manifestly, to all the other speculations which have the vortex atom, so to say, for their foundation-stone. Thus Professors Stewart and Tait's inferences as to the destructibility of matter,

based on the supposition that the ether is not quite frictionless; Professor Dolbear's suggestions as to the creation of matter through the development of new ether ripples, and the same thinker's speculations as to an upper limit of temperature, based on the mechanical conception of a limit to the possible vibrations of a vortex ring, not to mention other more or less fascinating speculations based on the vortex hypothesis, must be regarded, whatever their intrinsic interest, as insecurely grounded, until such time as new experimental methods shall give them another footing. Lord Kelvin himself holds all such speculations utterly in abeyance. "The vortex theory," he says, "is only a dream. Itself unproven, it can prove nothing, and any speculations founded upon it are mere dreams about a dream." [1]

That certainly must be considered an unduly modest pronouncement regarding the only workable hypothesis of the constitution of matter that has ever been imagined; yet the fact certainly holds that the vortex theory, the great contribution of the nineteenth century towards the solution of a world-old problem, has not been carried beyond the stage of hypothesis, and must be passed on, with its burden of interesting corollaries, to another generation for the experimental evidence that will lead to its acceptance or its refutation. Our century has given experimental proof of the existence of the atom, but has not been able to fathom in the same way the exact form or nature of this ultimate particle of matter.

Equally in the dark are we as to the explanation of that strange affinity for its neighbors which every atom

manifests in some degree. If we assume that the power which holds one atom to another is the same which in the case of larger bodies we term gravitation, that answer carries us but a little way, since, as we have seen, gravitation itself is the greatest of mysteries. But again, how chances it that different atoms attract one another in such varying degrees, so that, for example, fluorine unites with everything it touches, argon with nothing? And how is it that different kinds of atoms can hold to themselves such varying numbers of fellow-atoms—oxygen one, hydrogen two, and so on? These are questions for the future. The wisest chemist does not know why the simplest chemical experiment results as it does. Take, for example, a water-like solution of nitrate of silver, and let fall into it a few drops of another water-like solution of hydrochloric acid; a white insoluble precipitate of chloride of silver is formed. Any tyro in chemistry could have predicted the result with absolute certainty. But the prediction would have been based purely upon previous empirical knowledge—solely upon the fact that the thing had been done before over and over, always with the same result. Why the silver forsook the nitrogen atom and grappled the atom of oxygen no one knows. Nor can any one as yet explain just why it is that the new compound is an insoluble, colored, opaque substance, whereas the antecedent ones were soluble, colorless, and transparent. More than that, no one can explain with certainty just what is meant by the familiar word soluble itself. That is to say, no one knows just what happens when one drops a lump of salt or sugar into a bowl of water. We may believe with Professor Ostwald and his fol-

lowers that the molecules of sugar merely glide everywhere between the molecules of water, without chemical action; or, on the other hand, dismissing this mechanical explanation, we may say with Mendeleef that the process of solution is the most active of chemical phenomena, involving that incessant interplay of atoms known as dissociation. But these two explanations are mutually exclusive, and nobody can say positively which one, if either, is right. Nor is either theory at best more than a half explanation, for the why of the strange mechanical or chemical activities postulated is quite ignored. How is it, for example, that the molecules of water are able to loosen the intermolecular bonds of the sugar particles, enabling them to scamper apart?

But, for that matter, what is the nature of these intermolecular bonds in any case? And why, at the same temperature, are some substances held together with such enormous rigidity, others so loosely? Why does not a lump of iron dissolve as readily as the lump of sugar in our bowl of water? Guesses may be made to-day at these riddles, to be sure, but anything like tenable solutions will only be possible when we know much more than at present of the nature of intermolecular forces and of the mechanism of molecular structures. As to this last, studies are under way that are full of promise. For the past ten or fifteen years Professor Van 't Hoof of Amsterdam (now of Berlin), with a company of followers, has made the space relations of atoms a special study, with the result that so-called stereo-chemistry has attained a firm position. A truly amazing insight has been gained into the space

relations of the molecules of carbon compounds in particular, and other compounds are under investigation. But these results, wonderful though they seem when the intricacy of the subject is considered, are, after all, only tentative. It is demonstrated that some molecules have their atoms arranged in perfectly definite and unalterable schemes, but just how these systems are to be mechanically pictured—whether as miniature planetary systems or what not—remains for the investigators of the future to determine.

It appears, then, that whichever way one turns in the realm of the atom and molecule, one finds it a land of mysteries. In no field of science have more startling discoveries been made in the past century than here; yet nowhere else do there seem to lie wider realms yet unfathomed.

LIFE PROBLEMS

In the life history of at least one of the myriad star systems there has come a time when, on the surface of one of the minor members of the group, atoms of matter have been aggregated into such associations as to constitute what is called living matter. A question that at once suggests itself to any one who conceives even vaguely the relative uniformity of conditions in the different star groups is as to whether other worlds than ours have also their complement of living forms. The question has interested speculative science more perhaps in our generation than ever before, but it can hardly be said that much progress has been made towards a definite answer. At first blush the demonstration that all the worlds known to us are composed

of the same matter, subject to the same general laws, and probably passing through kindred stages of evolution and decay, would seem to carry with it the reasonable presumption that to all primary planets, such as ours, a similar life-bearing stage must come. But a moment's reflection shows that scientific probabilities do not carry one safely so far as this. Living matter, as we know it, notwithstanding its capacity for variation, is conditioned within very narrow limits as to physical surroundings. Now it is easily to be conceived that these peculiar conditions have never been duplicated on any other of all the myriad worlds. If not, then those more complex aggregations of atoms which we must suppose to have been built up in some degree on all cooling globes must be of a character so different from what we term living matter that we should not recognize them as such. Some of them may be infinitely more complex, more diversified in their capacities, more widely responsive to the influences about them, than any living thing on earth, and yet not respond at all to the conditions which we apply as tests of the existence of life.

This is but another way of saying that the peculiar limitations of specialized aggregations of matter which characterize what we term living matter may be mere incidental details of the evolution of our particular star group, our particular planet even—having some such relative magnitude in the cosmic order, as, for example, the exact detail of outline of some particular leaf of a tree bears to the entire subject of vegetable life. But, on the other hand, it is also conceivable that the conditions on all planets comparable in position to

ours, though never absolutely identical, yet pass at some stage through so similar an epoch that on each and every one of them there is developed something measurably comparable, in human terms, to what we here know as living matter; differing widely, perhaps, from any particular form of living being here, yet still conforming broadly to a definition of living things. In that case the life-bearing stage of a planet must be considered as having far more general significance; perhaps even as constituting the time of fruitage of the cosmic organism, though nothing but human egotism gives warrant to this particular presumption.

Between these two opposing views every one is free to choose according to his preconceptions, for as yet science is unable to give a deciding vote. Equally open to discussion is that other question, as to whether the evolution of universal atoms into a "vital" association mass from which all the diversified forms evolved, or whether such shifting from the so-called non-vital to the vital was many times repeated—perhaps still goes on incessantly. It is quite true that the testimony of our century, so far as it goes, is all against the idea of "spontaneous generation" under existing conditions. It has been clearly enough demonstrated that the bacteria and other low forms of familiar life which formerly were supposed to originate "spontaneously" had a quite different origin. But the solution of this special case leaves the general problem still far from solved. Who knows what are the conditions necessary to the evolution of the ever-present atoms into "vital" associations? Perhaps extreme pressure may be one of these conditions; and, for aught any man knows to the

contrary, the "spontaneous generation" of living protoplasms may be taking place incessantly at the bottom of every ocean of the globe.

This of course is a mere bald statement of possibilities. It may be met by another statement of possibilities, to the effect that perhaps the conditions necessary to the evolution of living matter here may have been fulfilled but once, since which time the entire current of life on our globe has been a diversified stream from that one source. Observe, please, that this assumption does not fall within that category which I mention above as contraband of science in speaking of the origin of worlds. The existence of life on our globe is only an incident limited to a relatively insignificant period of time, and whether the exact conditions necessary to its evolution pertained but one second or a hundred million years does not in the least matter in a philosophical analysis. It is merely a question of fact, just as the particular temperature of the earth's surface at any given epoch is a question of fact, the one condition, like the other, being temporary and incidental. But, as I have said, the question of fact as to the exact time of origin of life on our globe is a question that science as yet cannot answer.

But, in any event, what is vastly more important than this question as to the duration of time in which living matter was evolved is a comprehension of the philosophical status of this evolution from the "non-vital" to the "vital." If one assumes that this evolution was brought about by an interruption of the play of forces hitherto working in the universe—that the correlation of forces involved was unique, acting

then and then only—by that assumption he removes the question of the origin of life utterly from the domain of science—exactly as the assumption of an initial push would remove the question of the origin of worlds from the domain of science. But the science of to-day most emphatically demurs to any such assumption. Every scientist with a wide grasp of facts, who can think clearly and without prejudice over the field of what is known of cosmic evolution, must be driven to believe that the alleged wide gap between vital and non-vital matter is largely a figment of prejudiced human understanding. In the broader view there seem no gaps in the scheme of cosmic evolution—no break in the incessant reciprocity of atomic actions, whether those atoms be floating as a "fire mist" out in one part of space, or aggregated into the brain of a man in another part. And it seems well within the range of scientific expectation that the laboratory worker of the future will learn how so to duplicate telluric conditions that the universal forces will build living matter out of the inorganic in the laboratory, as they have done, and perhaps still are doing, in the terrestrial oceans.

To the timid reasoner that assumption of possibilities may seem startling. But assuredly it is no more so than seemed, a century ago, the assumption that man has evolved, through the agency of "natural laws" only, from the lowest organism. Yet the timidity of that elder day has been obliged by the progress of the past century to adapt its conceptions to that assured sequence of events. And some day, in all probability, the timidity of to-day will be obliged to

take that final logical step which to-day's knowledge foreshadows as a future if not a present necessity.

The Mechanism of the Cell

Whatever future science may be able to accomplish in this direction, however, it must be admitted that present science finds its hands quite full, without going farther afield than to observe the succession of generations among existing forms of life. Since the establishment of the doctrine of organic evolution, questions of heredity, always sufficiently interesting, have been at the very focus of attention of the biological world. These questions, under modern treatment, have resolved themselves, since the mechanism of such transmission has been proximately understood, into problems of cellular activity. And much as has been learned about the cell of late, that interesting microcosm still offers a multitude of intricacies for solution.

Thus, at the very threshold, some of the most elementary principles of mechanical construction of the cell are still matters of controversy. On the one hand, it is held by Professor O. Butschli and his followers that the substance of the typical cell is essentially alveolar, or foamlike, comparable to an emulsion, and that the observed reticular structure of the cell is due to the intersections of the walls of the minute ultimate globules. But another equally authoritative school of workers holds to the view, first expressed by Frommann and Arnold, that the reticulum is really a system of threads, which constitute the most important basis of the cell structure. It is even held that these

fibres penetrate the cell walls and connect adjoining cells, so that the entire body is a reticulum. For the moment there is no final decision between these opposing views. Professor Wilson of Columbia has suggested that both may contain a measure of truth.

Again, it is a question whether the finer granules seen within the cell are or are not typical structures, "capable of assimilation, growth, and division, and hence to be regarded as elementary units of structure standing between the cell and the ultimate molecules of living matter." The more philosophical thinkers, like Spencer, Darwin, Haeckel, Michael Foster, August Weismann, and many others, believe that such "intermediate units must exist, whether or not the microscope reveals them to view. Weismann, who has most fully elaborated a hypothetical scheme of the relations of the intracellular units, identifies the larger of these units not with the ordinary granules of the cell, but with a remarkable structure called chromatin, which becomes aggregated within the cell nucleus at the time of cellular division—a structure which divides into definite parts and goes through some most suggestive manœuvres in the process of cell multiplication. All these are puzzling structures; and there is another minute body within the cell, called the centrosome, that is quite as much so. This structure, discovered by Van Beneden, has been regarded as essential to cell division, yet some recent botanical studies seem to show that sometimes it is altogether wanting in a dividing cell.

In a word, the architecture of the cell has been shown by modern researches to be wonderfully com-

plicated, but the accumulating researches are just at a point where much is obscure about many of the observed phenomena. The immediate future seems full of promise of advances upon present understanding of cell processes. But for the moment it remains for us, as for preceding generations, about the most incomprehensible, scientifically speaking, of observed phenomena, that a single microscopic egg cell should contain within its substance all the potentialities of a highly differentiated adult being. The fact that it does contain such potentialities is the most familiar of every-day biological observations, but not even a proximal explanation of the fact is as yet attainable.

The Ancestry of the Mammals

Turning from the cell as an individual to the mature organism which the cell composes when aggregated with its fellows, one finds the usual complement of open questions, of greater or less significance, focalizing the attention of working biologists. Thus the evolutionist, secure as is his general position, is yet in doubt when it comes to tracing the exact lineage of various forms. He does not know, for example, exactly which order of invertebrates contains the type from which vertebrates sprang, though several hotly contested opinions, each exclusive of the rest, are in the field. Again, there is like uncertainty and difference of opinion as to just which order of lower vertebrates formed the direct ancestry of the mammals. Among the mammals themselves there are several orders, such as the whales, the elephants, and even man himself, whose exact lines of more immediate an-

cestry are not as fully revealed by present paleontology as is to be desired.

The New Science of Anthropology

All these, however, are details that hardly take rank with the general problems that we are noticing. There are other questions, however, concerning the history and present evolution of man himself that are of wider scope, or at least seemingly greater importance from a human stand-point, which within recent decades have come for the first time within the scope of truly inductive science. These are the problems of anthropology —a science of such wide scope, such far-reaching collateral implications, that as yet its specific field and functions are not as clearly defined or as generally recognized as they are probably destined to be in the near future. The province of this new science is to correlate the discoveries of a wide range of collateral sciences—paleontology, biology, medicine, and so on—from the point of view of human history and human welfare. To this end all observable races of men are studied as to their physical characteristics, their mental and moral traits, their manners, customs, languages, and religions. A mass of data is already at hand, and in process of sorting and correlating. Out of this effort will probably come all manner of useful generalizations, perhaps in time bringing sociology, or the study of human social relations, to the rank of a veritable science. But great as is the promise of anthropology, it can hardly be denied that the broader questions with which it has to deal—questions of race, of government, of social evolution—are still this side the fixed

plane of assured generalization. No small part of its interest and importance depends upon the fact that the great problems that engage it are as yet unsolved problems. In a word, anthropology is perhaps the most important science in the entire hierarchy to-day, precisely because it is an immature science. Its position to-day is perhaps not unlike that of paleontology at the close of the eighteenth century. May its promise find as full fruition!

IX

RETROSPECT AND PROSPECT

THE SCIENTIFIC ATTITUDE OF MIND

ANY one who has not had a rigid training in science may advantageously reflect at some length upon the meaning of true scientific induction. Various illustrations in our text are meant to convey the idea that logical thinking consists simply in drawing correct conclusions as to the probable sequence of events in nature. It will soon be evident to any one who carefully considers the subject that we know very little indeed about cause and effect in a rigid acceptance of these words. We observe that certain phenomena always follow certain other phenomena, and these observations fix the idea in our mind that such phenomena bear to one another the relation of effect and cause. The conclusion is a perfectly valid one so long as we remember that in the last analysis the words "cause" and "effect" have scarcely greater force than the terms "invariable antecedent" and "invariable consequent"—that is to say, they express an observed sequence which our experience has never contradicted.

Now the whole structure of science would be hopelessly undermined had not scientific men come to have the fullest confidence in the invariability of certain of

these sequences of events. Let us, for example, take the familiar and fundamental observation that any unsupported object, having what we term weight, invariably falls directly towards the centre of the earth. We express this fact in terms of a so-called law of gravitation, and every one, consciously or unconsciously, gives full deference to this law. So firmly convinced are we that the gravitation pull is a cause that works with absolute, unvarying uniformity that we should regard it as a miracle were any heavy body to disregard the law of gravitation and rise into the air when not impelled by some other force of which we have knowledge. Thanks to Newton, we know that this force of gravitation is not at all confined to the earth, but affects the whole universe, so that every two bits of matter, regardless of location, pull at each other with a force proportionate to their mass and inversely as the square of their distance.

Were this so-called law of gravitation to cease to operate, the entire plan of our universe would be sadly disarranged. The earth, for example, and the other planets would leave their elliptical orbits and hurtle away on a tangential course. We should soon be beyond the reach of the sun's beneficent influence; an arctic chill would pervade polar and tropical regions alike, and the term of man's existence would come suddenly to a close. Here, then, is a force at once the most comprehensible and most important from a human stand-point that can be conceived; yet it cannot be too often repeated, we know nothing whatever as to the nature of this force. We do not know that there may not be other starlike clusters beyond our

universe where this force does not prevail. We do not know that there may not come a period when this force will cease to operate in our universe, and when, for example, it will be superseded by the universal domination of a force of mutual repulsion. For aught we know to the contrary, our universe may be a pulsing organism, or portion of an organism, all the particles of which are at one moment pulled together and the next moment hurled apart—the moments of this computation being, of course, myriads of years as we human pygmies compute time.

To us it would be a miracle if a heavy body, unsupported, should fly off into space instead of dropping towards the centre of the earth; yet the time may come when all such heavy objects will thus fly off into space, and when the observer, could there be such, must marvel at the miracle of seeing a heavy object fall towards the earth. Such thoughts as these should command the attention of every student of science who would really understand the meaning of what are termed natural laws. But, on the other hand, such suggestions must be held carefully in check by the observation that scientific imagining as to what may come to pass at some remote future time must in no wise influence our practical faith in the universality of certain natural laws in the present epoch. We may imagine a time when terrestrial gravitation no longer exerts its power, but we dare not challenge that power in the present. There could be no science did we not accept certain constantly observed phenomena as the effect of certain causes. The whole body of science is made up solely of such observations and in-

ferences. Natural science is so called because it has to do with observed phenomena of nature.

NATURAL VERSUS SUPERNATURAL

A further word must be said as to this word "natural," and its complementary word "supernatural." I have said in an early chapter that prehistoric man came, through a use of false inductions, to the belief in supernatural powers. Let us examine this statement in some detail, for it will throw much light on our later studies. The thing to get clearly in mind is the idea that when we say "natural" phenomena we mean merely phenomena that have been observed to occur. From a truly scientific stand-point there is no preconception as to what manner of phenomenon may, or may not, occur. All manner of things do occur constantly that would seem improbable were they not matters of familiar knowledge. The simplest facts in regard to gravitation involve difficulties that were stumbling-blocks to many generations of thinkers, and which continue stumbling-blocks to the minds of each generation of present-day children.

Thus most of us can recall a time when we first learned with astonishment that the earth is "round like a ball"; that there are people walking about on the other side of the world with their feet towards ours, and that the world itself is rushing through space and spinning rapidly about as it goes. Then we learn, further, that numberless familiar phenomena would be quite different could we be transported to other globes. That, for example, a man who can spring two or three feet into the air here would be able, with the same muscular

exertion, to vault almost to the house-tops if he lived on a small planet like the moon; but, on the other hand, would be held prone by his own weight if transported to a great planet like Jupiter.

When, further, we reflect that with all our capacity to measure and estimate this strange force of gravitation we, after all, know absolutely nothing as to its real nature; that we cannot even imagine how one portion of matter can act on another across an infinite abysm (or, for that matter, across the smallest space), we see at once that our most elementary scientific studies bring us into the presence of inscrutable mysteries. In whatever direction we turn this view is but emphasized. Electricity, magnetism, the hypothetical ether, the inscrutable forces manifested everywhere in the biological field—all these are, as regard their ultimate nature, altogether mysterious.

In a word, the student of nature is dealing everywhere with the wonderful, the incomprehensible. Yet all the manifestations that he observes are found to repeat themselves in certain unvarying sequences. Certain applications of energy will produce certain movements of matter. We may not know the nature of the so-called cause, but we learn to measure the result, and in other allied cases we learn to reason back or infer the cause from observation of results. The latter indeed is the essence of scientific inquiry. When certain series of phenomena have been classified together as obviously occurring under the domination of the same or similar causes, we speak of having determined a law of nature. For example, the fact that any body in motion tends to go on at the same rate of

speed in a direct line forever, expresses such a law. The fact that the gravitation pull is directly as the mass and inversely as the square of the distance of the bodies it involves, expresses another such law. The fact that the planetary bodies of the solar system revolve in elliptical orbits under the joint influence of the two laws just named, expresses yet another law. In a word, then, these so-called "laws" are nothing more than convenient formulæ to express the classification of observed facts.

INDUCTIVE VERSUS DEDUCTIVE REASONING

The ancient thinkers indulged constantly in what we now speak of as deductive reasoning. They gave heed to what we term metaphysical preconceptions as to laws governing natural phenomena. The Greeks, for example, conceived that the circle is the perfect body, and that the universe is perfect; therefore, sun and moon must be perfect spheres or disks, and all the orbits of the heavenly bodies must be exactly circular. We have seen that this metaphysical conception, dominating the world for many centuries, exerted a constantly hampering influence upon the progress of science. There were numerous other instances of the same retarding influence of deductive reasoning. Modern science tries to cast aside all such preconceptions. It does not always quite succeed, but it makes a strenuous effort to draw conclusions logically from observed phenomena instead of trying to force observations into harmony with a preconeived idea. Herein lies the essential difference between the primitive method and the perfected modern method. Neither the one nor

the other is intended to transcend the bounds of the natural. That is to say, both are concerned with the sequence of actual events, with the observation of actual phenomena; but the modern observer has the almost infinite advantage of being able to draw upon an immense store of careful and accurate observations. A knowledge of the mistakes of his predecessors has taught him the value of caution in interpreting phenomena that seem to fall outside the range of such laws of nature as experience has seemed to demonstrate. Again and again the old metaphysical laws have been forced aside by observation; as, for example, when Kepler showed that the planetary orbits are not circular, and Galileo's telescope proved that the spot-bearing sun cannot be a perfect body in the old Aristotelian sense.

New means of observation have from time to time opened up new fields, yet with all the extensions of our knowledge we come, paradoxically enough, to realize but the more fully the limitations of that knowledge. We seem scarcely nearer to-day to a true understanding of the real nature of the "forces" whose operation we see manifested about us than were our most primitive ancestors. But in one great essential we have surely progressed. We have learned that the one true school is the school of experience; that metaphysical causes are of absolutely no consequence unless they can gain support through tangible observations. Even so late as the beginning of the nineteenth century, the great thinker, Hegel, retaining essentially the Greek cast of thought, could make the metaphysical declaration that, since seven planets were known, and since seven is the

perfect number, it would be futile to search for other planets. But even as he made this declaration another planet was found. It would be safe to say that no thinker of the present day would challenge defeat in quite the Aristotelian or Hegelian manner; but, on the other hand, it is equally little open to doubt that, in matters slightly less susceptible of tangible demonstration, metaphysical conceptions still hold sway; and as regards the average minds of our time, it is perhaps not an unfair estimate to say they surely have not advanced a jot beyond the Aristotelian stand-point. Untrained through actual experience in any field of inductive science, they remain easy victims of metaphysical reasoning. Indeed, since the conditions of civilization throw a protecting influence about us, and make the civilized man less amenable to results of illogical action than was the barbarian, it may almost be questioned whether the average person of to-day is the equal, as a scientific reasoner, of the average man of the Stone Age.

A few of the more tangible superstitions of primitive man have been banished from even the popular mind by the clear demonstration of science, but a host remains. I venture to question whether, if the test could be made in the case of ten thousand average persons throughout Christendom, it would not be found that a majority of these persons entertain more utterly mistaken metaphysical ideas regarding natural phenomena than they do truly scientific conceptions. We pride ourselves on the enlightenment of our age, but our pride is largely based on an illusion. Mankind at large is still in the dark age. The historian of the remote

future will see no radical distinction between the superstitions of the thirteenth century and the superstitions of the nineteenth century. But he will probably admit that a greater change took place in the world of thought between the year 1859 and the close of the nineteenth century than had occurred in the lapse of two thousand years before. If this estimate be correct, it is indeed a privilege to be living in this generation, for we are on the eve of great things, and beyond question the revolution that is going on about us denotes the triumph of science and its inductive method. Just in proportion as we get away from the old metaphysical preconceptions, substituting for them the new inductive method, just in that proportion do we progress. The essence of the new method is to have no preconceptions as to the bounds of nature; to regard no phenomenon, no sequence of phenomena, as impossible; but, on the other hand, to accept no alleged law, no theory, no hypothesis, that has not the warrant of observed phenomena in its favor.

The great error of the untrained mind of the primitive man was that he did not know the value of scientific evidence. He made wide leaps from observed phenomena to imagined causes, quite overlooking the proximal causes that were near to hand. The untrained observer of to-day makes the same mistake; hence the continued prevalence of those superstitious misconceptions which primitive man foisted upon our race. But each new generation of to-day is coming upon the field better trained in at least the rudiments of scientific method than the preceding generation, and this is perhaps the most hopeful feature of present-

day education. Some day every one will understand that there is no valid distinction between the natural and the supernatural; in fact, that no such thing as a supernatural phenomenon, in the present-day acceptance of the word, can conceivably exist.

All conceivable manifestations of nature are natural, nor can we doubt that all are reducible to law—that is to say, that they can be classified and reduced to systems. But the scientific imagination, as already pointed out, must admit that any and every scientific law of our present epoch may be negatived in some future epoch. It is always possible, also, that a seeming law of to-day may be proved false to-morrow, which is another way of saying that man's classification improves from generation to generation. For a "natural law," let it be repeated, is not nature's method, but man's interpretation of that method.

LOGICAL INDUCTION VERSUS HASTY GENERALIZATION

A great difficulty is found in the fact that men are forever making generalizations—that is, formulating laws too hastily. A few phenomena are observed and at once the hypothesis-constructing mind makes a guess as to the proximal causes of these phenomena. The guess, once formulated and accepted, has a certain influence in prejudicing the minds of future observers; indeed, where the phenomena involve obscure principles the true explanation of which is long deferred, a false generalization may impress itself upon mankind with such force as to remain a stumbling-block for an indefinite period. Thus the Ptolemaic conception of the universe dominated the thought of Europe for a

thousand years, and could not be substituted by the true theory without a fierce struggle; and, to cite an even more striking illustration, the early generalizations of primitive man which explain numberless phenomena of nature as due to an influence of unseen anthropomorphic beings remain to this day one of the most powerful influences that affect our race—an influence from which we shall never shake ourselves altogether free until the average man—and particularly the average woman—learns to be a good observer and a logical reasoner.

Something towards this end is being accomplished by the introduction of experimental research and scientific study in general in our schools and colleges. It is hoped that something towards the same end may be accomplished through study of the history of the development of science. Scarcely anything is more illuminative than to observe critically the mistakes of our predecessors, noting how natural the mistakes were and how tenaciously they were held to, how strenuously defended. Most of all it would be of value to note that the false inductions which have everywhere hampered the progress of science have been, from the stand-point of the generation in which they originated, for the most part logical inductions. We have seen that the Ptolemaic scheme of the universe, false though it was in its very essentials, yet explained in what may be termed a thoroughly scientific fashion the observed phenomena. It is one way of expressing a fact to say that the sun moves across the heavens from the eastern to the western horizon; and for most practical purposes this assumption answers perfectly. It is only when we

endeavor to extend the range of theoretical astronomy, and to gain a correct conception of the mechanism of the universe as a whole, that the essentially faulty character of the geocentric conception becomes apparent.

And so it is in many another field; the false generalizations and hasty inductions serve a temporary purpose. Our only quarrel with them is that they tend through a sort of inertia to go forever unchanged. It requires a powerful thrust to divert the aggregate mind of our race from a given course, nor is the effect of a new impulse immediately appreciable; that is why the masses of the people always lag a generation or two behind the advanced thinkers. A few receptive minds, cognizant of new observations that refute an old generalization, accept new laws, and, from the vantage-ground thus gained, reach out after yet other truths. But, for the most part, the new laws thus accepted by the leaders remain unknown to the people at large for at least one or two generations. It required about a century for the heliocentric doctrine of Copernicus to begin to make its way.

In this age of steam and electricity, progress is more rapid, and the greatest scientific conception of the nineteenth century, the Darwinian theory, may be said to have made something that approaches an absolute conquest within less than half a century. This seems a marvellously sudden conquest, but it must be understood that it is only the crude and more tangible bearings of the theory that have thus made their way. The remoter consequences of the theory are not even suspected by the great majority of those who call

themselves Darwinians to-day. It will require at least another century for these ideas to produce their full effect. Then, in all probability, it will appear that the nineteenth century was the most revolutionary epoch by far that the history of thought has known. And it owes this proud position to the fact that it was the epoch in all history most fully subject to the dominant influence of inductive science. Thanks to this influence, we of the new generation are able to start out on a course widely divergent from the path of our ancestors. Our leaders of thought have struggled free from the bogs of superstition, and are pressing forward calmly yet with exultation towards the heights.

APPENDIX

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CHAPTER IV

SOME PHYSICAL LABORATORIES AND PHYSICAL PROBLEMS

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CHAPTER VII

SOME MEDICAL LABORATORIES AND MEDICAL PROBLEMS

[1] (p. 185). Dr. Duclaux, who was one of Pasteur's chief assistants, and who succeeded him in the directorship of the In-

stitute, died in 1903. He held a professorship in the University of Paris during the later years of his life, and his special studies had to do largely with the chemical side of bacteriology.

CHAPTER VIII

SOME UNSOLVED SCIENTIFIC PROBLEMS

[1] (p. 217). Lord Kelvin's estimate as quoted was expressed to the writer verbally. I do not know whether he has anywhere given a similar written verdict.

A LIST OF SOURCES

I.—PERIOD COVERED BY VOLUME I.

ANAXAGORAS. See vol. i., p. 240.

ARCHIMEDES. See vol. i., p. 196.

Many of the works of Archimedes are lost, but the following have come down to us: (1) *On the Sphere and Cylinder;* (2) *The Measure of the Circle;* (3) *Conoids and Spheroids;* (4) *On Spirals*; (5) *Equiponderants and Centres of Gravity;* (6) *The Quadrature of the Parabola;* (7) *On Bodies Floating in Liquids;* (8) *The Psammites;* (9) *A Collection of Lemmas.*

ARISTARCHUS. See vol. i., p. 212.

Magnitudes and Distances of the Sun and Moon is the only surviving work. In the *Armarius* of Archimedes another work of Aristarchus is quoted—the one in which he anticipates the discovery of Copernicus. Delambre, in his *Histoire de l'astronomie ancienne*, treats fully the discoveries of Aristarchus.

ARISTOTLE. See vol. i., p. 82.

An edition of Aristotle was published by Aldus, Venice, 1495–1498, 5 vols. During the following eighty years seven editions of the Greek text of the entire works were published, and many Latin translations.

BEROSUS. See vol. i., p. 58.

The fragments of Berosus have been trans. by I. P. Cory, and included in his *Ancient Fragments of Phœnician, Chaldean, Egyptian, and Other Writers*, London, 1826; second edition, 1832.

DEMOCRITUS. See vol. i., p. 161.

Fragments only of the numerous works ascribed to Democritus have been preserved. *Democriti Abderitæ operum fragmenta*, Berlin, 1843, edited by F. G. A. Mullach.

DIODORUS SICULUS. See vol. i., p. 77.

The Historical Library. Perhaps the best available editions of Diodorus are Wesseling's, 2 vols.; Amstel, 1745; and Dindorf's, 5 vols., Leipzig, 1828–1831. English trans. by Booth, London, 1700.

DIOGENES LAERTIUS. See vol. i., p. 121.

The Lives and Opinions of Eminent Philosophers (trans. by C. D. Yonge), London, 1853.

ERATOSTHENES. See vol. i., p. 225.

The fragments of his philosophical works were published at Berlin, 1822, under the title *Eratosthenica*. His poetical works were published at Leipzig, 1872.

EUCLID. See vol. i., p. 193.

His *Elements of Geometry* is still available as an English school text-book.

GALEN (Claudius Galenus). See vol. i., p. 272.

Galen's preserved works are exceedingly bulky. The best-known edition is that of C. G. Kühn, in 21 volumes.

HERO. See vol. i., p. 242.

The Pneumatics of Hero of Alexandria, from the original Greek. Trans. by B. Woodcroft, London, 1851.

HERODOTUS. See vol. i., p. 103.

History. English trans. by Beloe, 1791 and 1806. Trans. by Canon Rawlinson, London, 1858–1860.

HIPPARCHUS. See vol. i., p. 233.

The only work of Hipparchus which has survived was published first by Vittorius at Florence, 1567.

HIPPOCRATES. See vol. i., p. 170.

Numerous editions have been published of the Hippocratic writings, including many works not written by the master himself. One of the best editions is that of Littré, Paris, 1839, etc.

KHAMURABI, CODE OF. See vol. i., p. 76.

This famous inscription is on a block of black diorite nearly eight feet in height. It was discovered at Susa by the French expedition under M. de Morgan in December, 1901.

LEUCIPPUS. See vol. i., p. 161.

PLINY (Caius Plinius Secundus). See vol. i., p. 265.

His *Natural History* is available in several English editions and reprints. Perhaps the best edition of the original text is the one published by Julius Sillig, 5 vols., Leipzig, 1854–1859.

PLUTARCH. See vol. i., p. 198.

Life of Marcellus, in *Parallel Lives*. In this the mechanical inventions of Archimedes are described.

POLYBIUS. See vol. i., p. 201.

In his *Histories* Polybius describes the mechanical contrivances and war-engines of Archimedes, and also gives an account of his death.

PTOLEMY (Claudius Ptolemaeus). See vol. i., p. 269.

Geographia (or *Almagest* of the Arabs). The edition published by Nobbe, in 3 vols., Leipzig, 1842, was one of the best complete editions of the Greek text. The edition published in Didot's *Bibliotheca Classicorum Græcorum*, Paris, 1883, is excellent. Earlier editions contain many errors.

STRABO. See vol. i., p. 255.

The Geography of Strabo. Trans. by H. C. Hamilton and W. Falconer, 3 vols., London, 1857. There are several other editions of Strabo's work available in English.

TERTULLIAN. See vol. i., p. 195.

Apologeticus.

THEOPHRASTUS. See vol. i., p. 188.

Περὶ φυτῶν ἱστορίας, *On the History of Plants*. Written in 10 books. This is one of the earliest works on botany which have come to us. It was largely used by Pliny. In complete works, Schneider, Leipzig, 1818–1821, 5 vols. *On Plants*, edited by Wimmer, Breslau,

1842–1862. *On Plants*, edited by Slackhouse, Oxford, 1814. Περὶ φυτῶν αἰτιῶν, *On the Causes of Plants*. This was originally in 8 books, of which 6 are now existant. Bibliog. *vid. History of Plants*.

II.—PERIOD COVERED BY VOLUME II.

ALBATEGNIUS, MOHAMMED BEN JABIR. See vol. ii., p. 15.

The original MS. of his principal work, *Zidje Sabi*, is in the Vatican. A Latin translation was first published by Plato Tiburtinus at Nuremberg, in 1537, under the title *De scientia stellarum*. Various reprints of this have been made.

ALBERTUS MAGNUS. See vol. ii., p. 127.

Philosophiæ Naturalis Isagoge, Vienna, 1514.

ALHAZEN (full name, Abu Ali al-Hasan Ibn Alhasan). See vol. ii., p. 18.

Only two of his works have been printed, his *Treatise on Twilight* and his *Thesaurus opticae*, these being available in Michael Casiri's *Bibliotheca Arabico-Hispana Escurialensis*, 2 vols., Madrid, 1760–1770.

BACON, FRANCIS. See vol. ii., p. 192.

Novum Organum was published in London, 1620. *The Letters and Life of Lord Bacon*, in 7 vols., by James Spedding, appeared in 1862–1874.

BACON, ROGER. See vol. ii., p. 44.

Only an approximate estimate of the number of Bacon's works can be given even now, although an infinite amount of time and labor has been spent in collecting them. His great work is the *Opus majus*, "the Encyclopædia and the Organum of the Thirteenth Century." A partial list of some of his other works is the following: *Speculum alchemiæ*, 1541 (trans. into English); *De mirabili potestate artis et naturæ*, 1542 (trans, into English, 1659); *Libellus de retardanis senectutis accidentibus*, 1590 (trans. as "The Cure of Old Age," 1683); and *Sanioris medicinæ Magistri d. Rogeri Baconis Anglici de arte chymiæ scripta*, 1603.

BOYLE, ROBERT. See vol. ii., p. 205.
Philosophical Works, 3 vols., London, 1738.

COPERNICUS, NICOLAUS. See vol. ii., p. 54.
Ad clar. v. d. Schonerum de libris revolutionum eruditiss. viri et mathematici excellentiss. Rev. Doctoris Nicolai Copernici Torunnaei, Canonici Warmiensis, per quemdam juvenem mathematicæ studiosum, Narratio prima, Dantzic, 1540. This was the first published statement of the doctrine of Copernicus, and was a letter published by Rheticus. Three years afterwards Copernicus's *De orbium cælestium revolutionibus, Libri VI.*, was published at Nuremberg (1543).

DESCARTES, RENÉ. See vol. ii., p. 193.
Traitē de l'homme (Cousins's edition, in 11 vols., Paris, 1824).

GALILEI, GALILEO. See vol. ii., p. 91.
Dialogo dei due massimi sistemi del mondo, Florence, 1632. *Discorsi e dimostrazioni matematiche intorno a due nuove scienze*, Leyden, 1638.

GILBERT, WILLIAM (1540–1603). See vol. ii., p. 113.
De magnete, magneticisque corporibus, et de magno magnete tellure, London, 1600. *De magnete* was trans. by P. Fleury Motteley, London, 1893.

GUERICKE, OTTO VON (1620–1686). See vol. ii., p. 213.
Experimenta nova, ut vocant, Magdeburgica de vacuo spatio, Amsterdam, 1672. In the *Phil. Trans.* of the Royal Society of London, No. 88, for 1672.

HALES, STEPHEN (1677–1761). See vol. ii., p. 298.
Statical Essays, comprising *Vegetable Staticks*, London, 1727, and *Hæmostatics*, London, 1733.

HARVEY, WILLIAM. See vol. ii., p. 169.
Exercitatio anatomica de motu cordis et sanguinis, Frankfort-on-Main, 1628. *The Works of*, trans. by Robert Willis, London, 1847.

HAUKSBEE, FRANCIS. See vol. ii., p. 259.
Physico-Mechanical Experiments on Various Subjects, London, 1709. This contains descriptions of his various

discoveries in electricity, many of which are given in the *Phil. Trans.*

HOOKE, ROBERT. See vol. ii., p. 215.

Micrographia, or Some Philosophical Descriptions of Some Minute Bodies, London, 1665. *An Attempt to Prove the Motion of the Earth*, London, 1674. *Microscopical Observations*, London, 1780. Most of Hooke's important discoveries were contributed as papers to the Royal Society and are available in the *Phil. Trans.*

HUYGENS, CHRISTIAN (1629–1695). See vol. ii., p. 218.

Traité de la lumière, Leyden, 1690. Complete works were published at The Hague in 1888, under thetit le *Œuvres complètes*, by the Société Hollandaise des Sciences. These books have not been translated into English. Huygens's famous paper on the laws governing the collision of elastic bodies appeared in the *Phil. Trans.* of the Royal Society for 1669.

KEPLER, JOHANN. See vol. ii., p. 70.

Astronomia nova de motibus stellæ Martis, Leipzig, 1609, contains Kepler's two first laws; and *Harmonices mundi*, 1619, contains the third law, *Phœnomenon singulare, seu Mercurius in sole*, Leipzig, 1609. *Joannis Kepleri opera omnia*, in 8 vols., Frankfort, 1858–1871.

LEEUWENHOEK, ANTHONY VAN. See vol. ii., p. 179.

His discoveries are mostly recorded in the *Phil. Trans.* of the Royal Society, between the years 1673 and 1723—one hundred and twelve papers in all. His discovery of bacteria is recorded in *Phil. Trans.* for 1683; and that of the discovery of the capillary circulation of the blood in *Phil. Trans.* for 1790.

LINNÆUS, CAROLUS (1707–1778). See vol. ii., p. 299.

His *Systema naturæ* was published in 1735. Two years later (1737) he published *Genera plantarum*, which is generally considered as the starting-point of modern botany. His published works amount to more than one hundred and eighty.

MARIOTTE, EDME (died 1684). See vol. ii., p. 210.

Essais de physique (four essays), Paris, 1676–1679.

His *De la nature de l'air*, containing his statement of the law connecting the volume and pressure of a gas, is contained in the second essay.

NEWTON, SIR ISAAC. See vol. ii., p. 241.

Philosophiæ naturalis principia mathematica, completed in July of 1687. The first edition was exhausted in a few months. There are several translations, among others one by Andrew Motte, New York, 1848.

PARACELSUS. See vol. ii., p. 159.

The Hermetic and Alchemical Writings of Paracelsus, trans. by A. E. Waite, 2 vols., London, 1894.

PASCAL, BLAISE. See vol. ii., p. 122.

Récit de la grande expérience de l'équilibre de liqueurs, Paris, 1648.

SAWTREE, JOHN. See vol. ii., p. 124 ff.

Of the Philosopher's Stone, London, 1652.

SWAMMERDAM, JOHN. See vol. ii., p. 297.

Bibel der Natur, trans. into German, Leipzig, 1752.

SYDENHAM, THOMAS. See vol. ii., p. 189.

His first work, *Methodus curandi febres*, was published in 1666. His last work, *Processus integri*, appeared in 1692. His complete works, in Latin, were published by the Sydenham Society, London, 1844, which published also an English translation by Dr. R. G. Latham in 1848. There are several other English translations.

TORRICELLI, EVANGELISTA. See vol. ii., p. 120.

Opera geometrica, Florence, 1644.

TYCHO BRAHE. See vol. ii., p. 65.

De mundi aetherei recentioribus phænomenis, Prague, 1603. This has been trans. into German by M. Bruns, Karlsruhe, 1894.

VINCI, LEONARDO DA. See vol. ii., p. 47.

Leonardo da Vinci, Artist, Thinker, and Man of Science, by Eugene Müntz, 2 vols., New York, 1892, is perhaps the most complete treatment of all phases of Leonardo's

work as a scientist as well as an artist. The older French work, *Essai sur les ouvrages physico-mathématiques de Léonard de Vinci*, by J. B. Venturi, Paris, 1797, is excellent. In German, H. Grothe's *Leonardo da Vinci als Ingenieur und Philosoph*, Berlin, 1874, is good.

III.—MODERN COSMICAL AND TELLURIC SCIENCES

Agassiz, L. See vol. iii., p. 147.

Études sur les glaciers, Neuchâtel, 1840.

Arago, François J. D. See vol. iii., p. 67.

Œuvres (complete), 17 vols., Paris, 1854–1862. *Arago's Meteorological Essays*, trans. into English, London, 1855. This has an introduction by Humboldt.

Boscovich, Roger Joseph. See vol. iii., p. 293.

Theoria philosophiæ naturalis redacta ad unicam legem virium in natura existentium, Vienna, 1758.

Bradley, James. See vol. iii., p. 13.

Concerning an Apparent Motion Observed in Some of the Fixed Stars, London, 1748, *Phil. Trans.*, vol. xlv., pp. 8, 9.

Cuvier, Baron de. See vol. iv., p. 103.

Recherches sur les ossements fossiles de quadrupèdes, 4 vols., Paris, 1812. (The introduction to this work was translated and published as a volume bearing title of *Theory of the Earth*, New York, 1818.)

Delambre, Jean Baptiste Joseph. See vol. iii., p. 16.

Histoire d'astronomie, Paris, 1817–1821. This work contains not only the history of the discoveries in astronomy, but is also a complete text-book of astronomy as understood at this period.

Falconer, Hugh. See vol. iii., p. 99.

In *Palæontological Memoirs*, vol. ii., pp. 596–598.

HERSCHEL, WILLIAM. See vol. iii., p. 20 ff.
On the Proper Motion of the Solar System, Phil. Trans., vol. 73, for 1783. (This paper was read in March, 1783.) *The Constitution of the Heavens, Phil. Trans.* for 1785, vol. 75, p. 213.

HOWARD, LUKE. See vol. iii., p. 182.
Philosophical Magazine, 1803.

HUMBOLDT, ALEXANDER VON. See vol. iii., p. 192.
Des lignes isothermes et de la distribution de la chaleur sur le globe, published in vol. iii., of *Mêmoires de physique et de chimie de la Société d'Arcueil*, Paris, 1819.

HUTTON, JAMES. See vol. iii., p. 178.
Theory of Rain, in *Transactions of the Royal Society of Edinburgh*, 1788, vol. i., pp. 53–56. See vol. iii., p. 121. From *Transactions of the Royal Society of Edinburgh*, 1788, vol. i., pp. 214–304. A paper on the "Theory of the Earth," read before the society in 1781.

KANT, IMMANUEL (1724–1804). See vol. iii., p. 27.
Allgemeine Naturgeschichte und Theorie des Himmels, 1755. *Cosmogony*, ed. and trans. by W. Hartie, D.D., Glasgow, 1900.

LAPLACE, M. LE MARQUIS DE. See vol. iii., p. 32.
Exposition du système du monde, Paris, 1796, is available in *Œuvres complètes*, in 12 vols., Paris, 1825–1833, vol. vi., p. 498.

LYELL, CHARLES. See vol. iii., p. 88.
Principles of Geology, 4 vols., London, 1834.

MARSH, O. C. See vol. iii., p. 107.
Fossil Horses in America (reprinted from *American Naturalist*, vol. viii., May, 1874), pp. 288, 289.

PLAYFAIR, JOHN. See vol. iii., pp. 131, 165.
Illustrations of the Huttonian Theory, 1802.

SCROPE, G. POULETT. See vol. iii., p. 132.
Consideration of Volcanoes, London, 1823, pp. 228–234.

WELLS, W. C. See vol. iii., p. 185.
Essay on Dew, London, 1818.

IV.—MODERN PHYSICAL AND CHEMICAL SCIENCES

BLACK, JOSEPH. See vol. iv., p. 12.
De acido e cibis orlo, et de magnesia, reprinted at Edinburgh, 1854. In this he sketched his discovery of carbonic acid. Later this paper was incorporated in his *Experiments on Magnesia, Quicklime, and Other Alkaline Substances*.

BUNSEN, WILLIAM. See vol. iv., p. 69.

CAVENDISH, HENRY. See vol. iv., p. 15.
"Experiments on Air," in *Phil. Trans.*, 1784, p. 119. This paper contains Cavendish's discovery of the composition of water and of nitric acid.

DAGUERRE, LOUIS J. M. See vol. iv., p. 70.
Historique et description des procédés du daguerréotype et du diorama, Paris, 1839. (This was translated into English.)

DALTON, JOHN. See vol. iv., p. 40.
"On the Absorption of Gases by Water," read before the Literary and Philosophical Society of Manchester, October 21, 1803. This was published in 1805, and contains the atomic weight of twenty-one substances, some of which were probably added, or corrected, between the date of the first reading and the publication.

DAVY, SIR HUMPHRY. See vol. iv., pp. 48, 209.
"Some Chemical Agencies of Electricity," in *Phil. Trans.* for 1806, vol. viii. *Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide or Dephlogisticated Nitrous Air and its Respiration*, London, 1800.

DEWAR, JAMES. See vol. v., p. 39.
"Solid Hydrogen," in *Proc. Roy. Inst.* for 1900. "The Nadir of Temperature and Allied Problems" (Bakerian Lecture), *Proc. Roy. Soc.*, 1901.

DUFAY, CISTERNAY. See vol. ii., p. 267.
Histoire de l'Académie Royale des Sciences, between 1733 and 1737, contains Dufay's principal papers.

EULER, LEONARD (1707–1783). See vol. iii., p. 17.
Lettres à une Princesse d'Allemagne sur quelques sujets de physique et de philosophie, St. Petersburg, 1768.

FARADAY, MICHAEL. See vol. iii., p. 241.
On the Induction of Electric Currents, in *Phil. Trans.* of Royal Society for 1832, pp. 126–128. *Explication of Arago's Magnetic Phenomena*, by Michael Faraday, F.R.S., *Phil. Trans.* of Royal Society for 1832, pp. 146–149.

FRANKLIN, BENJAMIN. See vol. ii., p. 286.
New Experiments and Observations on Electricity, London, 1760.

GALVANI, LUIGI (1737–1798). See vol. iii., p. 229.
De viribus electricitatis in motu musculari commentatio, Bologna, 1791. This discovery of Galvani was first brought to notice by Volta's famous paper to the Royal Society, entitled "An Account of some Discoveries made by Mr. Galvani, of Bologna," published in the *Phil. Trans.* for 1793, pp. 10–44.

GAY-LUSSAC, JOSEPH LOUIS. See vol. iv., p. 41.
Mémoire sur la combinaison des substances gazeuses, Mem. Soc. d'Arcueil, 1809.

HALLEY, EDMUND. See vol. iii., p. 7.
An Account of Several Extraordinary Meteors or Lights in the Sky, in *Phil. Trans.*, vol. xxix., pp. 159 – 162, London, 1714.

HELMHOLTZ, H. L. F. See vol. iii., p. 280.
Handbuch der physiologische Optik, Leipzig, 1867.

JOULE, J. P. See vol. iii., p. 269.
On the Calorific Effects of Magneto-Electricity and the Mechanical Value of Heat, in *Report of the British Association for the Advancement of Science*, 1843, vol. xii., p. 33.

KIRWAN, R. See vol. iv., p. 3 ff.

An Essay on Phlogiston and the Constitution of Acids, London, 1789. This is interesting, written as it was just before Lavoisier's *Elements* treated the same subject from the stand-point of the anti-phlogistic chemists.

KLEIST, DEAN VON. See vol. ii., p. 280.

In the *Danzick Memoirs*, vol. i. contains the description given by Von Kleist of his discovery of the Leyden jar. A translation is given also in Priestley's *History of Electricity*.

LAVOISIER, ANTOINE LAURENT. See vol. iv., p. 33.

Traité élémentaire de chimie, Paris, 1774, trans. as *Elements of Chemistry*, by Robert Kerr, London and Edinburgh, 1790.

LISTER, JOSEPH JACKSON. See vol. iv., p. 113.

On Some Properties in Achromatic Object Glasses Applicable to the Improvement of the Microscope, in *Phil. Trans.* for 1830.

MAXWELL, JAMES CLERK-. See vol. iii., p. 45.

"On the Motions and Collisions of Perfectly Elastic Spheres" in *Philosophical Magazine* for January and July, 1860. *The Scientific Papers of J. Clerk-Maxwell*, edited by W. D. Nevin (2 vols.), vol. i., pp. 372–374, Cambridge, 1896. This is a reprint of Maxwell's prize paper of 1859.

MAYER, DR. JULIUS ROBERT. See vol. iii., p. 259.

The Forces of Inorganic Nature, 1842. This is Mayer's statement of the conservation of energy.

MENDELÈEFF, DMITRI IVANOVITCH. See vol. iv., p. 68.

Principles of Chemistry, 2 vols., London, 1868–1870. (There have been several subsequent editions.)

OERSTED, HANS CHRISTIAN. See vol. iii., p. 236.

Experiments with the Effects of the Electric Current on the Magnetic Needle, published at Berlin, 1816.

PRIESTLEY, JOSEPH. See vol. iv., pp. 20, 36.

Experiments and Observations on Different Kinds of Air, 3 vols., Birmingham, 1790. *History of Electricity*,

vol. ii., p. 280, London, 1775. *The Doctrine of Phlogiston Established*, 1800.

RAMSAY AND RAYLEIGH. See vol. v., p. 86.

"On an Anomaly Encountered in Determining the Density of Nitrogen Gas," in *Proc. Roy. Soc.*, April, 1894. A statement of the properties of argon was made by the discoverers to the Royal Society, given in *Phil. Trans.*, clxxxvi., p. 187, January, 1895.

SCHEELE, KARL WILLIAM. See vol. iv., p. 23.

Om Brunsten, eller Magnesia, och dess Egenakaper, Stockholm, 1774. This contains his discovery of chlorine. His book, *Chemische Abhandlung von der Luft und dem Feuer*, was published in 1777.

THOMPSON, BENJAMIN (Count Rumford). See vol. iii., p. 208.

Essays Political, Economical, and Philosophical (2 vols.), vol. ii., pp. 470 – 493, London, T. Cadell, Jr., and W. Davies, 1797.

THOMSON, WILLIAM (Lord Kelvin). See vol. iii., p. 276.

On a Universal Tendency in Nature to the Dissipation of Mechanical Energy, in *Transactions of the Royal Society of Edinburgh*, 1852.

WOLLASTON, WILLIAM HYDE. See vol. iv., p. 41.

Phil. Trans. for 1814, vol. civ., p. 1, contains a synoptic scale of chemical equivalents. This paper was confirmatory of Dalton's theory.

YOUNG, THOMAS. See vol. iii., p. 218.

On the Colors of Thin Plates, l.c. in *Phil. Trans.* for 1802, pp. 35–37.

V.—MODERN BIOLOGICAL SCIENCES

AVENBRUGGER, LEOPOLD. See vol. iv., p. 200.

Inventum novum ex percussione thoracis humani interni pectoris morbos detegendi, Vienna, 1761.

BELL, SIR CHARLES See vol. iv., p. 249.
An Exposition of the Natural System of Nerves of the Human Body, being a Republication of the Papers delivered to the Royal Society on the Subject of the Nerves in 1811, etc.
BERNARD, CLAUDE. See vol. iv., p. 137.
BOERHAAVE, HERMANN. See vol. iv., p. 182.
Institutiones medicæ, Leyden, 1708; and *De chemie expurgante suos errores*, Lugduni Batavorum, 1718.
BROWN, ROBERT. See vol. iv., p. 115.
On the Organs and Mode of Fecundation of Orchideæ and Asclepiadeæ, in *Miscellaneous Botanical Works*, London, 1866.

CHAMBERS, ROBERT. See vol. iv., p. 161.
Vestiges of the Natural History of Creation, London, 1844 (published anonymously). His *Sequel to Vestiges* was published a year later.
CHARCOT, JEAN MARTIN. See vol. iv., p. 269.
Leçons sur les maladies du système nerveux, Paris, beginning in 1873.
CUVIER, GEORGE, BARON DE. See vol. iv., p. 159.
Histoire naturelle des animaux sans vertebres, Paris, 1815. *Système des connaissances positives de l'homme*, Paris, 1820.

DARWIN, ERASMUS. See vol. iv., pp. 94, 147.
The Botanic Garden, London, 1799. *The Temple of Nature, or The Origin of Society*, edition published in London, 1807.
DARWIN, CHARLES. See vol. iii., p. 95, and vol. iv., p. 173.
The Origin of Species, London, 1859.

FECHNER, GUSTAV. See vol. iv., p. 263.
Elemente du Psychophysik, 1860.
FLOURENS, MARIE JEAN PIERRE. See vol. iv., p. 270.
Experiences sur le système nerveux, Paris, 1825. *Cours sur la génération, l'ovologie, et l'embryologie*, Paris, 1836, etc.

GALL, FRANZ JOSEPH. See vol. iv., p. 248.
Recherches sur le système nerveux en général, et sur

celui du cerveau en particulier, Paris, 1809. (This paper was laid before the Institute of France in March, 1808.)

GOETHE, JOHANN WOLFGANG. See vol. iv., p. 140.

Die Metamorphose der Pflanzen, 1790.

GRAY, STEPHEN. See vol. ii., p. 262.

Most of his original papers appeared in the *Phil. Trans.* between 1720 and 1737.

HAECKEL, ERNST HEINRICH. See vol. v., p. 144.

Natürlich Schopfungsgeschichte, 1866, rewritten in a more popular style two years later as *Natural History of Creation.* Some of his more important monographs are: *Radiolaria* (1862), *Siphonophora* (1869), *Monera* (1870), *Calcarious Sponges* (1872), *Arabian Corals* (1876), another *Radiolaria*, enumerating several thousand new species, accompanied by one hundred and forty plates (1887), and *Die Welträthsel*, trans. in 1900 as *The Riddle of the Universe.*

HAHNEMANN, WILHELM VON. See vol. iv., p. 189.

Organon der rationellen Heilkunde, Dresden, 1810.

HALL, MARSHALL, M.D., F.R.S.L. See vol. iv., p. 251.

On the Reflex Functions of the Medulla Oblongata and the Medulla Spinalis, in *Phil. Trans.* of Royal Society, vol. xxxiii., 1833.

HUNTER, JOHN. See vol. iv., p. 92.

On the Digestion of the Stomach after Death, first edition, pp. 183–188.

JENNER, EDWARD. See vol. iv., p. 190.

An Inquiry into the Causes and Effects of the Variolæ Vaccinæ, London, 1799.

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American Museum of Natural History Bulletins, New York.

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Annales medico-psychologiques, Paris.

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